

Suspended Animation in Atomic Energy

AT the beginning of this month, the United Kingdom Atomic Energy Authority divested itself of two important functions, the manufacture, sale and reprocessing of nuclear fuel and the sale of radioisotopes. From April 1, these activities have been respectively the responsibilities of autonomous companies, British Nuclear Fuels Limited and Radiochemical Centre Limited. For the time being, of course, it is improbable that customers will notice much difference in the operations of the two enterprises, which remain wholly owned subsidiaries of the Atomic Energy Authority, although Sir John Hill, the chairman, has let it be known that an attempt to estimate the commercial value of the companies is being made by a merchant bank and that, in due course, it may be possible to sell off some of the shares. There is a natural interest among, for example, mining companies in such a shareholding and even though the proposal to split off the two companies was originally devised by the Labour government in the best Fulton spirit, the Conservative government is likely to seize eagerly on whatever opportunities may arise for translating this public investment into cash. The possibility that the Labour government may have been wildly optimistic in its calculations about the ease with which partnerships between public and private capital might be established is at this stage a red herring—only time will tell but it is almost certain that in the year ahead the Conservative government will seek to implement the proposal in the recent White Paper on the reorganization of defence procurement that the nuclear weapons laboratory at Aldermaston should be transferred to the Ministry of Defence.

In strictly arithmetical terms, close on half of the authority's financial interest may, in other words, have been hived off by the end of 1971. What is likely to happen to the rest? More than a year has gone by since Mr Wedgwood Benn's grand design for the incorporation of the Atomic Energy Authority into a new corporation responsible for industrial research and development fell flatly on the ears of those most directly concerned, the directors of government research establishments, the Atomic Energy Authority itself and even the industrial companies considered on the face of things to be the most likely beneficiaries. Although it may have been unfair to complain, as many people did at the time, that Mr Wedgwood Benn's scheme was merely a device for keeping an existing organization in being, yet the falling through of that set of proposals does necessarily imply that some other disposition of the authority's resources must now be made. It is unthinkable that the Atomic Energy Authority should be allowed to continue as a loose federation of laboratories which happen to be simply those which have not already been dispensed with.

What are the authority's present functions? The most important of its present functions is that of carrying out the research and development necessary to make a workable fast reactor system. The construction of a prototype reactor at Dounreay in Scotland, due to be finished in 1972, has been contracted out to the Nuclear

Power Group Limited, but the authority remains responsible for more advanced research and development. Luckily, the early belief on the authority's part that it would be possible not merely to delegate the construction of this prototype but almost to forget about research and development in this field has been put aside, and the authority now seems to have a sensible measure of the importance that such a crucial part of the future development of British industry should be considered as a long-term public responsibility. If there is a complaint about the authority's present policy on fast reactors, it is that too little has been done to coordinate and even integrate this research with what is being done elsewhere in Europe. (Is this not one of the issues to which more attention should have been given in the recent skirmishes about British and Scandinavian membership of the European Economic Community?) Evidently there will be a great deal to be done even after 1974, the earliest date by which the Central Electricity Generating Board can place an order for a fast reactor. All the experience of nuclear energy suggests that fast reactor designs will continue to improve well into the eighties, and there is obviously a case for a small but vigorous research organization devoted almost exclusively to the task of seeing that the work is properly planned and executed. In short, an atomic energy authority of some kind must continue to exist.

It does not follow from this conclusion that the rest of the authority as at present constituted must remain unchanged. When Aldermaston is transferred to the Ministry of Defence, the likely and desirable course of action, the laboratories at Winfrith and Harwell will seem even more anomalously placed than they are at present. The former is now largely occupied with development work on existing reactor types—the advanced gas cooled reactor and the high temperature reactor—which could and should be the responsibility of the industrial companies hoping to make money from the sale of equipment like that. For practical purposes, the laboratory's work is partly a hidden subsidy of commercial enterprise. Is there a good reason why this should continue? Although there is no case at all for a drastic and sudden change of circumstances at Winfrith, the government—this government above all—should pay close attention to intermediate arrangements for helping development in the newish nuclear power industry. Why not, for example, a system of bridging development contracts by means of which reluctant companies might be persuaded that their best interests really do lie in the continuing improvement of existing reactor designs?

The problem of Harwell is much more difficult and much more interesting. For nearly four years now, the laboratory has had a considerable success in attracting research contracts from private industry. By now, about a quarter of its work must consist either of commissioned research or of research programmes not directly related to atomic energy which have been sanctioned by the government. At the same time the part played by the laboratory



in British scientific and technical life is considerable, and extends far beyond its influence in the development of nuclear power as such. Indeed, the most serious difficulty in deciding what should become of the Atomic Energy Research Establishment at Harwell is that its present role is much more like that of a national physical laboratory than a research organization devoted simply to one industry. So does not logic or at least good sense require that the laboratory should be thought of in such a light? In other words, why should not the Atomic Energy Research Establishment and the National Physical

Laboratory be combined? The result would be an organization with much more in common with the National Bureau of Standards in the United States than any British organization has at present. For one thing, it would be capable of carrying out the work with standards which is a necessary part of any government's responsibility. But it would also be able to function as a general catalyst of industrial development. The Harwell establishment in particular would be enormously enlivened if it were no longer typecast by its original interest in nuclear energy.

How Not to Use Technology

THE latest development in the farce which has prevented the British Overseas Airways Corporation from using its new fleet of six Boeing 747s is a dispute over rates of pay for flight engineers. According to the union, it will take three weeks before the results will be available of a poll in which the engineers are being asked their opinions on the acceptability of a pay offer for engineers who fly in the 747. For the airline, of course, this delay is especially galling because it comes after a year of frustration when the six aircraft have been sitting solemnly on the ground, their engines on hire to other airlines whose 747s were not grounded by a failure to agree on pay for pilots. So far, the airline estimates that it has lost £7 million in interest charges and lost revenue, and even if there has recently been a tendency among discriminating airline passengers to elect to cross the Atlantic in aircraft which are not jumbo-sized, thus blunting a little BOAC's deprivation, the latest delay in using the 747s is bound to be a serious matter. No wonder the airline has been talking of laying off men because it is at present prevented from winning revenue from its investment in the new Boeing. The sad truth, unfortunately, is that even when the aircraft are able to take the air with fare-paying passengers, there is a serious danger that they will be unprofitable. The overriding question is not so much the practical issue of putting the 747s in service as the philosophical question of how to devise machinery for allowing airlines to operate without cutting their own throats.

These are hard times for airlines everywhere. Last year, airlines in the United States made a total loss of \$120 million, while European airlines, BOAC among them, found that their profitability was decreased. What has happened is that the airlines have for the past five years been counting on a growth of passenger traffic which has not in the event materialized. The Boeing 747 is for practical purposes designed to fit the transatlantic route, where in the late sixties traffic seemed to be growing at about 20 per cent a year. Some growth has been maintained, but not nearly enough to fill the seats in all the jumbo jets. In the third quarter of last year, for example, the average load factor on the 747s owned by the two airlines then operating these machines across the Atlantic was just over 60 per cent. This is a poor performance for the peak of the annual pilgrimage east and west across the Atlantic. Since then, aircraft have been making the journey more than half empty. BOAC this week found no difficulty in cramming the passengers from a flightless 747 into a conventional jet aircraft, with plenty of room to spare. The sky above the Atlantic is clearly filled with

aircraft in which passengers can take substantial promenades if that is their inclination. Unhappily for the airline, this state of affairs is no joke. The direct cost of operating a Boeing 747 is close on £500 an hour, while interest charges on the cost of buying the aircraft are also substantial.

In the circumstances, it is no surprise that BOAC and Pan American Airways have in the past few days been setting up a clamour for still lower fares across the Atlantic. What they want is what any owner of under-used equipment seeks—a chance to attract more business. The difficulty with which they must contend is that airline fares are regulated by the price fixing cartel called the International Air Transport Association, one of the more dubious of the organizations sponsored by the United Nations. The result is that both BOAC and Pan American, in their latest attempts to sell transatlantic tickets at lower prices, are forced to think of unnatural expedients by means of which intending passengers should qualify. BOAC, for example, suggests that its lower fare should be offered only to those who book in advance and pay in advance. This is a device that may have the side effect of improving the airline's cash position but it is of course a foolish restriction to place on one of the essential commodities of the modern world—the availability of cheap air transport. It would be much better if the airlines were allowed to charge lower fares for transatlantic flights at inconvenient times, for this would allow them not merely to make fuller use of their aircraft but also to balance more sensitively the demand for offpeak travel.

Stratagems like these are, however, only palliatives. What the airlines most urgently need is liberation from the straitjacket within which they are at present forced to operate. First of all, there should be some measure of freedom for airlines to fix fares at levels which seem to them to be appropriate, which implies at least a modification of the IATA regulations and even an abandonment of the whole system. It is true, of course, that one awkward consequence of this would be that airlines would be tempted to undercut each other, which is as efficient a method of self-destruction as over-investment in new aircraft, so that some form of price fixing will always remain. The difference between the present system and that most to be desired is that the fares fixed by IATA are almost by definition those which do not drive the least efficient carriers out of business. The result is that on busy routes such as the North Atlantic, more than a dozen airlines are competing for such traffic as may exist. It goes without saying that more freedom on fares would

also require greater freedom for individual airlines to fly to international airports outside their own territory, which would entail some change of the present system under which airline routes are negotiated between governments with all the solemnity of disarmament treaties, but that would only be another advantage. Indeed, if the airlines

could win for themselves more freedom on fares and routes, it would be well worth their clinching with governments an agreement that public money would not in future be used to subsidize the operation of international airlines. That is the third ingredient necessary to allow the economics of airline operations to find its proper level.

Time to Close the English Channel

In the past few weeks, the English Channel has become less of a seaway than a hazard to shipping. Mariners and civil servants are fond of saying that the channel is the busiest shipping route in the world and this, indeed, seems to be a fact. Several hundred ships pass each way each day and one measure of the rate at which the traffic in the channel is increasing is the rate at which collisions seem to be occurring. On the face of things, of course, it would be reasonable to expect that the frequency of collision would be proportional to the square of the volume of traffic, or at least to the product of the volume of traffic in each direction. In practice, however, the nature of the channel and the rules which exist for regulating traffic there make the probability of collision much greater. First, sandbanks in the channel do necessarily confine ships to comparatively narrow tracks. Second, the rules promulgated by the shipping authorities for segregating opposing traffic into separate streams—west-bound hard against the English coast and east bound on the other side—also require that opposing streams of traffic should cross at certain critical places. The result is that the problem of predicting collisions in the channel is not so much like the problem of predicting collisions between billiard balls on a smooth billiard table as that of predicting collisions between motor vehicles without brakes at road intersections without traffic lights. A little reflexion will show how fortunate it is that there has been comparatively little damage in the channel in the past few months—three vessels sunk, two damaged and one aground on the Goodwin Sands.

In spite of all the argument between Trinity House (which maintains lighthouses and lightships in British waters), the Admiralty and the shipowners about the best routing of traffic in the English Channel, there is every prospect that things will get worse before they can possibly become better. For one thing, there is an ambitious plan to develop a new port at the place at which it is almost certain that the government will be forced to choose for the third London Airport—Foulness in the Thames Estuary. Second, the Goodwin Sands (just two miles off the Kent coast) and the other underwater obstacles to navigation are likely to become more conspicuous as time goes on, if only because the channel continues to serve as a means by which solid material from the Atlantic can be deposited in the North Sea. In short, the time will come when the best way of avoiding mutual shipping damage in the channel will be to do away with the shipping. But even as things are, much of the shipping exists only because the channel exists. If there were a dam from Dover to Calais, or if the natural causeway which disappeared 6,000 years ago with the melting of the ice were still intact, cross-channel traffic would go by dry land, the great ports of Europe would probably lie nearby

but Scandinavia would still be as close to South America because the route round Scotland to the northern ports is probably now as short in time as that through the Straits of Dover. So does it not make sense to ask whether the time has now arrived for getting rid of the English Channel, preferably by a dam that would yield electricity as well as a means of transport. By now, after all, it seems clear that the channel tunnel will not be built. In January, the British and French governments agreed on yet another device for postponing action—a scheme to spend £10 million on a further programme of research and development unlikely to be finished before 1974. The two governments are right in their scepticism about the economic viability of a tunnel. If they wait long enough, they may even find that the channel clogs up without deliberate intervention with the hulls of all the ships likely to be sunk there by collisions in the next few years.

100 Years Ago



PANGENESIS

(Extract from a leader)

When, therefore, Mr. Galton concludes from the fact that rabbits of one variety, with a large proportion of the blood of another variety in their veins, do not produce mongrelised offspring, that the hypothesis of Pangenesis is false, it seems to me that his conclusion is a little hasty. His words are, "I have now made experiments of transfusion and cross circulation on a large scale in rabbits, and have arrived at definite results, negating, in my opinion, beyond all doubt the truth of the doctrine of Pangenesis." If Mr. Galton could have proved that the reproductive elements were contained in the blood of the higher animals, and were merely separated or collected by the reproductive glands, he would have made a most important physiological discovery. As it is, I think every one will admit that his experiments are extremely curious, and that he deserves the highest credit for his ingenuity and perseverance. But it does not appear to me that Pangenesis has, as yet, received its death blow; though, from presenting so many vulnerable points, its life is always in jeopardy; and this is my excuse for having said a few words in its defence. CHARLES DARWIN

From *Nature*, 3, 502, April 27, 1871.

OLD WORLD

RESEARCH FINANCE

Nuffield's Money

THE trustees of the Nuffield Foundation, one of the largest private financiers of scientific, medical, educational and social projects in Britain and the British Commonwealth, received only guarded reassurance of the safety of their investments at the annual general meeting of the British Leyland Motor Corporation last Monday. The shareholders were still smarting from the recent news that the dividend for 1970 will be down from 11 per cent to only $3\frac{3}{4}$ per cent. This results from a fall in profits (before tax) from more than £40 million in 1969 to less than £4 million in 1970, a fall which the BLMC attributes partly to the rising cost of materials but chiefly to the interruption of production and loss of output caused by industrial disputes of unprecedented intensity.

The Nuffield Foundation was established in 1946 with an endowment from Lord Nuffield of shares to a total value of £10 million in what was then Morris Motor Holdings. Because of mergers these have since been converted into shares in British Leyland company, but the amount of the investment remains the same and today represents nearly 70 per cent of the foundation's total investment. This loss of the BLMC dividend means that the income of £1.5 million normally available for grants in any one year will be reduced in 1971 to something less than £700,000, and if the BLMC's interim dividend, which is due next August, falls below its usual $3\frac{3}{4}$ per cent, this figure could be even lower.

In itself, this setback need not necessarily be cause for alarm. The foundation maintains a reserve of unallocated income which could be used in just such an emergency. Last year this fund stood at over £500,000, and this year is likely to be more. Once it is used, however, it must somehow be replaced. At the same time, British Leyland has promised that as soon as its earnings increase to an adequate level, it will recommend higher than usual dividends to counteract the shareholders' recent losses.

This is a fair promise, but what are the prospects of a reversal of the present downhill trend? Lord Stokes, chairman of British Leyland, said at last Monday's meeting that production during the first six months of its current financial year has been much better than in the corresponding period last year, but went on to point out that, even so, increasing costs are constantly eroding the profit margin. This is the situation faced, not by some small concern, but

by one of the largest companies in the United Kingdom, with an annual turnover of £1,000 million and the biggest exporter in the country. Last year's loss of £16 million by the Austin Morris division was the most serious of the corporation's setbacks, and clearly, the expectations of a brighter future here rest squarely on the ADO 28, the new Morris car, whose name and form are to be released next Tuesday. It represents the first original venture of the corporation since it was formed by the merger of Leyland and British Motor Corporation in 1968.

Even if the Nuffield Foundation's difficulties are overcome, however, that the situation can arise at all will undoubtedly cause concern. Although the foundation has never claimed to be the mainstay of any one area of research, it does provide funds for otherwise unsupported but promising lines of new investigation. With so much pioneering work at stake is it, therefore, wise for the administrators of the largest section of the foundation's resources to use it as what seems in part to be an immovable memorial to Lord Nuffield's faith in the British motor industry? Should any charitable trust jeopardize beneficiaries as yet unknown by investing most of its money in too tight and therefore too vulnerable a corner of the stock market? Certainly the other investments of the Nuffield Foundation are spread over a very wide range of companies in an effort to guard against the failure of any one of them as well as to maximize the revenue. Diversifying the shares at present held in British Leyland would at least avoid the incongruous situation faced by many British and Commonwealth researchers, who must now pin their hopes of future support on the success of a new car.

SPACE TRAVEL

Once More in Orbit

from our Soviet Correspondent

THE launching of the Soviet *Salyut* spacecraft, one step towards the creation of the permanently manned orbital station envisaged by Tsiolkovskii, seems to have been carefully timed not only to coincide with celebrations of the tenth anniversary of Gagarin's flight but also, by following closely upon the 24th Congress of the Communist Party of the Soviet Union, to reinforce the directives of the new five-year plan. They include "the acceleration of the rate of growth of scientific and technical progress, by means of multi-lateral development of research in the most promising fields of science".

The launching of *Salyut* was by no means unexpected. In his speech on the Gagarin anniversary last week,

Academician Mstyslav V. Keldysh, President of the Soviet Academy of Sciences, stressed that the recent *Soyuz* experiments, the 18-day mission of *Soyuz* 9, were important preliminary stages in the programme for such a station. As a result, he said, it had become clear that the problem of readaptation to Earth conditions after a long stay in space requires the serious attention of scientists. Writing in *Pravda*, in connexion with the Gagarin anniversary, Professor V. Pavlov was even more explicit. "In future long-term flights", he said, "it will be necessary to imitate to a maximum degree on board space stations the effect of terrestrial gravity on the human organism, either by creating artificial gravity, or by loading all the groups of muscles". In the same article, Pavlov indicated that the current Soviet space programme envisages manned projects in Earth orbit only; remoter exploration is to be carried out by unmanned probes.

PHYSICS EXHIBITION

Changing with the Times

THE annual Physics Exhibition organized by the Institute of Physics was last week held once again in the inelegant Alexandra Palace, London. In general, 1971 has been a year of continuing difficulty for the exhibition industry, and in many ways things are even more awkward for the Physics Exhibition which has in the past been run as a non-commercial venture to show off new ideas from industry and academic institutions. The institute does, however, seem to have begun to adapt its exhibition to the rather chilly economic climate which has forced many firms and institutions both to cut down on research and development and to examine very closely the channels through which they publicize and sell their products.

Many would consider that during the last seven or eight years the institute has adopted an excessively purist attitude which has ultimately led to rather more of a decline in the number of exhibitors than might otherwise have been the case. In 1963, for example, the exhibition reached its peak with 150 companies taking part, but by 1970 this number had been cut by more than 40 per cent. Some of this reduction can be traced back to the disappearance of many companies by mergers, but in spite of this, the decline has been real enough. Fortunately, however, the number of government laboratories, nationalized industries and academic institutions exhibiting has increased and they now represent about 40 per cent of exhibitors, compared with 25 per cent in 1968. It is hardly surprising

that the number of academic exhibitors has remained buoyant because the institute allows them a certain amount of free bench space, makes no page charge for entries in the handbook and makes some contribution to out-of-pocket expenses.

Just before the 1970 exhibition, the worrying lack of industrial exhibitors prompted the setting up of a working party to make recommendations for improving the situation. The working party decided that certain changes could be made without destroying the essential *raison d'être* of the exhibition and the chief of these has been put into practice at the 1971 exhibition. This is the creation of a new category of exhibits which do not have to be refereed and which can be shown for commercial reasons alone. The number in this new category is, however, restricted because exhibitors must have at least one refereed exhibit accepted before they are allowed to display products in the second category and even then, no more than twice the number of refereed exhibits is allowed. This commercial category accounts for about 130 of the 490 exhibits at this week's exhibition, but the number of companies exhibiting has only shown a slight rally and is still less than a hundred.

In many ways, the institute is following the example of the French Physical Society but with some reservations. The French Physics Exhibition, held in Paris, at about 18-monthly intervals, is about the closest to the British exhibition in concept and aims. The French have for several years adopted a policy of allowing commercial exhibits and exhibitors to display only one novel device or instrument in order to be allowed to exhibit any number of commercial items. Although the presentation of the French exhibition is of a high standard, it does suffer to some extent from an over emphasis on commercialism. At the other extreme, the scientific exhibitions held in the United States bear little resemblance to either the French or the British. Both the Physics Show, which forms an integral part of the annual meeting of the American Institute of Physics, and the Electronics Show run by the Institute of Electrical and Electronic Engineers are principally commercial functions; but even these displays have shown signs of erosion in the last year or two.

In spite of several years of apparently depressing exhibition statistics, the Physics Exhibition is basically quite sound financially, although a superficial look at the accounts of the Institute of Physics can give a misleading impression. Until 1968, the excess of exhibition expenditure over income was rarely more than £100 or so but since

then, losses of about £4,000 have been recorded. The explanation of this sudden downturn in the fortunes of the exhibition is, however, nothing to do with the exhibition itself but simply reflects a change in the institute's accounting procedure—the exhibition now has to shoulder a proportion of the normal overheads. One of the necessary consequences of this development (which is equivalent, of course, to requiring that the exhibition itself make a profit) is that charges for bench space at the 1971 exhibition had to be raised by about 30 per cent. This was obviously an awkward thing to have to do while trying to attract exhibitors, but it does ensure that the 1971 exhibition will break even.

LEARNED SOCIETIES

Geologists on the Rocks

THE Geological Society of London, the oldest company of geologists in the world, has fallen upon hard times and there are stern lessons to be learned from its present plight. When learned societies in general are suffering from a deceleration in the rapid rate of expansion which took place after the Second World War, the performance of the geologists stands out as being particularly poor. Although finance is clearly the most important and serious of the society's worries, there are also signs that it is losing its appeal for many of its present and potential supporters.

In 1968, the society's balance sheet showed a deficit of almost £800, and this grew to more than £1,500 by 1969. The accounts for 1970 show that this deficit is now running close to £2,400, and that the accumulated fund, the reserve which functions to cushion the society against financial shocks has fallen to only £12,784. These are dismal disclosures for an organization whose total income from subscriptions is only £22,000 and there are now clear indications that the disquiet which many of the fellows have felt in the past about the society's affairs is making itself plain in concrete terms. But it would seem that the geologists must tread a flinty path for the next few years, before their present efforts to atone for their past mistakes show signs of success.

The obvious reasons for the geologists' financial discomfort are plain enough; they have attempted to diversify their activities too rapidly and on too grand a scale from a base which is becoming increasingly precarious. It would be hard to find fault with the council's plans for the future development of the society except, perhaps, to say that they are not exciting enough; but the roots of the problem run very much deeper than that.

The chief cause seems to have been a degree of lethargy in the running of the society's affairs which has resulted in failure to keep up to date with the more controversial trends in modern geology—indeed the Royal Astronomical Society (RAS) now seems to have arrogated to itself the role of keeper of the geological conscience in such matters as plate tectonics and sea floor spreading.

But another cause for concern has been the depressing delay which has characterized the Geological Society's publication record in recent years. In some cases, communications have been published as much as two years after the date on which they were first received. Learned societies rely heavily on finance from publications, which, together with subscriptions, constitute the bulk of their income.

This is well illustrated in the accounts of societies such as the RAS and the Royal Statistical Society (RSS). The publications account of the RAS for the year 1969, for example, shows a surplus of more than £10,000 which comfortably offsets the deficit of £3,899 on the general account. In comparison, the most recent figures for the Geological Society show that it spent over £21,000 on publications in 1970 to bring in £12,485, a sad and indefensible loss.

The society is looking in two chief directions in belated attempts to restore its fortunes. First, the entire publication programme has been overhauled. To this end, responsibility for production of the society's official organ, the *Quarterly Journal*, has been handed over to Scottish Academic Press, so that responsibility only for the scientific content of the journal remains with the society. The *Quarterly Journal* itself will soon close, and will be replaced by a new publication which will appear six times a year.

The second line of attack is to repair the society's dowdy image. There are newly created specialist groups in engineering geology, volcanic studies and marine studies. Sedimentology, micropalaeontology and structural geology will soon be added. The hope is that this move will help to rejuvenate membership, and reverse the present trend indicated by the large numbers of fellows who fail to pay their dues and are removed from the society's list.

Although the geologists have special problems, most learned societies are having to prepare themselves for squalls ahead. Finance is not always the most urgent problem; professional societies, for example the chemists, are frequently very wealthy, and the RAS and the RSS are good examples of organizations who can balance their books. The necessity to plan ahead for suitable accommodation, however, is high on the list of priorities in the minds of many secretaries.

NEW WORLD

Water Gets Worse

by our Washington Correspondent

A SCATHING indictment of the attempts made by government and industry to abate water pollution in the United States has been compiled by a student task force working at the direction of the consumer advocate Ralph Nader. On the basis of apparently unrestricted access to the files of the relevant federal agencies, the task force describes the government's almost total failure to bring about any significant improvement in water pollution during the past 15 years. Reasons for the failure include unenforceable and toothless laws, unconscionable delays by industrial polluters, foot dragging by local and state authorities, insufficient clean-up funds provided by federal government, use of positions in the Federal Water Quality Administration for political patronage, and a diffuse and unfruitful programme of research into water pollution control.

This catalogue of woes* is the fruit of nearly two years' research by 25 people, many of them law students at Harvard University. The findings of the report, which was published last week, have been as good as admitted by Mr William D. Ruckelshaus, the Administrator of the Environmental Protection Agency to which responsibility for water pollution control has now been transferred. "Our agency is dedicated to nothing less than the restoration of those waters that have been subjected to the abominable abuse that is chronicled in the Nader report," Ruckelshaus conceded in a statement last week. The report may conceivably have some stiffening effect on the water pollution legislation now going through Congress.

"The effectiveness to date [of the federal role in water pollution control] can be concisely assessed by the virtual absence of any evidence that the seven laws passed and more than \$3 billion spent by the federal government have reduced the level of pollution in any major body of water," Ralph Nader observes in a preface to the report. Since US manufacturers in 1969 invested an average of 0.18 per cent of their gross revenue in air and water pollution control equipment, this lack of progress is understandable. But it surprised an emissary of President Johnson who in 1965, ten years after the start of the water pollution control programme, was sent to compile a list of five places where water quality had

been improved as an example of achievements under the Great Society.

The Federal Water Quality Administration spent a week running salinity counts and computer studies but could find not a single river where its activities had secured an improvement in water quality. Asking the same question this year, the Nader task force found that the FWQA "still does not attach enough importance to the question of water use improvement to have bothered to consolidate at headquarters the information needed to answer it," but there were only three places where officials thought the situation might have improved, one of which, Lake Michigan, seems in point of fact to have deteriorated.

cipal pollution. This has been tackled by numerous grants to industry and universities as well as by a large in-house research effort at eight laboratories around the country. The field should have been ripe for advance since the technological base of the sanitary engineering profession has lagged far behind scientific advances in other fields, the task force comments, and since "the profession's psychology has been conservative, and its technology is a grab bag of stop-gap measures taken to control pollution in the early 20th century."

Asked to identify the new ideas the FWQA has brought to pollution control, the director of research, Dr Stephan, could only point to some

FEDERAL WATER POLLUTION CONTROL RESEARCH EXPENDITURES
(in millions of dollars)

	In-house operations	Research grants and contracts	Demonstration projects	Total
Before 1966	16.7	18.3	2.7	37.7
1966-1970	46.6	50.5	111.9	209.0
Total	63.3	68.8	114.6	246.7

One reason, though not the most important, for this lack of progress is the dearth of achievement from the research efforts of the Federal Water Quality Administration (now the Water Quality Office of the Environmental Protection Agency). While paying tribute to the scientific excellence of the director of the research programme, Dr David Stephan, the Nader report claims that the research has produced little of value: "After ten years, and the expenditure of over \$240 million, the research effort at FWQA has failed to provide a single significant technological innovation which has been implemented on a large scale and is now being used to control water pollution in this country."

Congress, the task force declares, has little incentive for scrutinizing the research budget of the agency since large research budgets allow officials to claim they are doing something about water pollution without the unpleasant head-on confrontations with industry that regulatory activities produce. In the past five years the research budget of the Federal Water Pollution Control Administration has averaged about \$44 million a year, or half of the agency's total budget.

The emphasis of the R and D programme has been on problems of muni-

"relatively obvious adaptations of the old chemical engineering technology which existed long before the research programme got underway", none of which has yet been applied to any significant degree in practical pollution control situations. Out of 59 FWQA-supported research projects dealing with phosphate removal by coagulation and precipitation in 1969, no fewer than six demonstrate the efficacy of sodium aluminate, an agent whose use for this purpose had already reached the textbooks in 1952.

"You cannot call a research programme successful because it duplicates the discoveries of 20 years ago," the task force acidly reports. In respect of Dr Stephan's boasts for the FWQA's development of methods of tertiary treatment for sewage, the task force points out that one of the techniques tried by the FWQA, rapid sand filtration, was the subject of a patent in the early 1880s. Another tertiary technique, carbon absorption, was described in the summary report of the agency for 1961 as "probably the most likely contender for a position in the technology of advanced waste treatment"; the technique has been used for purifying water in laboratories since the late nineteenth century.

Only one technique which the FWQA

* *Water Wasteland*, edited by David R. Zwick and Marcy Benstock. Center for Study of Responsive Law, Washington DC, 1971. \$25.

has worked on can be considered a genuine innovation—a process of biological denitrification in which the ammonia in sewage is converted first to nitrate and then to nitrogen gas by two species of bacteria maintained in optimal conditions in separate compartments. But as of last month, this promising process has not been used in a single full-scale operating waste treatment plant.

Another false promise of the agency's research programme is what the task force report dubs the "Great Phosphorus Removal Hoax", a salutary tale that is possibly not without parallels elsewhere. In 1966 the Department of the Interior (in which the agency was then situated) issued a press release claiming that research in the removal of phosphates from sewage heralded a "major and early breakthrough in water pollution". Nothing again was ever heard of the breakthrough and the project concerned has since been cancelled.

What prompted the announcement to the press? The task force was informed by an unnamed FWQA official that the FWQA laboratory at Ada, Oklahoma, needed more money since its funds were being cut at the time. Researchers at the laboratory persuaded the South Central Regional Director of the FWQA, William Gallagher, to cable Washington with the news that they believed the phosphate removal problem was solved; all that was needed, the cable said, was money to support extensive field tests. The Ada laboratory got its money, but within a year the project was revealed as a hoax.

"In ten years of FWQA research, no real progress towards the scientific solution of water pollution has occurred—very little new information, no breakthroughs, no application of knowledge," is the verdict of the Nader task force. Perhaps because of this lack of substantive progress, there has been a tendency for funds to be shifted from research to demonstration projects (see table). But the demonstration projects, intended to prove to polluters that pollution can be controlled, are based on the fundamental misconception that only a lack of technical knowledge is what hinders polluters from installing the necessary equipment.

The Nader report recommends that spending on such demonstration projects should be sharply curtailed. Research grants to industry to study industrial pollution problems should also be cancelled except in exceptional circumstances. "The best government research programme", the report concludes, is a strong enforcement programme to generate a demand for practical new pollution control ideas. . . . The commitment to control pollution must come first; only then will the solutions follow."

FIRE ANTS

Fire Ants Defended

by our Washington Correspondent

THE Department of Agriculture has successfully fought off an injunction by the Environmental Defense Fund which seeks to prohibit a programme for controlling the fire ant in the southern United States. The court decision, given last week, is a disappointment for environmentalists who after a battle of several months had hoped to force the abandonment of the project altogether. But the programme now sanctioned represents a drastic scaling down of the original project, a plan that sought to eradicate the fire ant from nine southern states by spraying some 130 million acres three times over with a persistent chlorinated hydrocarbon pesticide known as Mirex.

The programme the department has now settled for, and successfully defended before a Washington DC court, is only a control programme under which some 4.5 million acres will receive a single application of the pesticide. The real reasons for undertaking the programme are obscure; although the fire ant may not be quite as harmless as some environmentalists have claimed, it is hard to see that it is a menace worthy of a 12-year, \$200 million eradication programme that the Department of Agriculture had conceived for it until recently. Plans to eradicate the ant continued despite a report delivered by a committee of the National Academy of Sciences on September 28, 1967, which stated that complete eradication was not biologically or technically feasible and that even if it were the committee had grave doubts if it would be desirable.

Why was the department so eager to get rid of the fire ant? The ant, known to entomologists as *Solenopsis saevissima*, was introduced into the southern United States from South America sometime around 1918 and has since infested about 130 million acres in nine southern states. Although it is thought now to have reached the northern limit of its range, entomologists believe it could spread westward to California. The fire ant has a painful sting and is a hazard to workers at harvest time. The large mounds it constructs in open fields can also interfere with harvesting and damage equipment. According to a survey undertaken by the Department of Agriculture, the methodology of which is derided by environmentalists, the agricultural damage caused in this way by the fire ant amounts to \$11 million a year.

Mirex, the pesticide chosen by the department to control the ant, is the chlorinated hydrocarbon dodecachlorooctahydro-1,3,4-metaheno-2Hcyclobuta (cd)pentalene. Spread from the air in

the form of a bait and at a concentration of 1.7 grams an acre, Mirex is picked up by fire ants and taken to their nests. The chemical is also known to kill other species of ant which are attracted by the bait.

Conservationists have objected to the Department of Agriculture's eradication project on the grounds that the introduction of vast quantities of persistent hydrocarbon pesticide into the environment was likely to prove a mixed blessing for a comparatively unimportant effect. They pointed out that Mirex is toxic to young fish and crustaceans and is known to be carcinogenic in laboratory animals. As with DDT, residues of Mirex accumulate in the body of non-target organisms in the treated areas and become concentrated in the higher levels of food chains. These objections formed the basis for an injunction taken out against the Department of Agriculture by the Environmental Defense Fund and other conservation groups.

Denying the injunction last week, District Court Judge Oliver Gasch said that in his opinion the Department of Agriculture was doing all that was required of it under the National Environmental Policy Act of 1969. Scientists in the department had "sufficiently studied the potential impact of widespread use of Mirex on the environment and the Environmental Impact Statement prepared by the department set forth in sufficient detail the expected environmental effects of the fire ant control programme". The plaintiffs, the judge said, "have not demonstrated that they are likely to succeed in proving the allegations of their complaint".

The fire ant control programme against which the judge denied the injunction represents a substantial retreat from the original eradication project. The Department of Agriculture, however, is disguising its precipitate retreat as a victory, claiming that the goal of eradication was only discussed as a proposal from the southern states and never accepted as a formal plan. The reason for reducing the size of the programme, which is a cost-sharing venture between the federal government and the states concerned, is that the states have been reluctant to put up their share of the money. One factor behind the states' newly found parsimony may have been "all the screaming by the environmental people", a department official admitted last week.

Despite the reduction in the scale of the programme, the environmental people are less than fully happy with the decision. Charles F. Wurster, an Assistant Professor of Environmental Sciences at the State University of New York who has served as Consultant to the Environmental Defense Fund, says

that the environmental impact statement prepared by the Department of Agriculture is ridiculous, since it offers no evaluation of the carcinogenicity of Mirex or of its effects on predator avian enzyme systems and egg shells. Serious reservations about the Mirex programme are apparent in the comments of the Department of Health, Education and Welfare and the Environmental Protection Agency, to which the USDA impact statement was submitted. But on the basis of 8 years' experience with Mirex, during which no non-target organism except for related species of oil-loving ants have suffered proven harm, the Department of Agriculture is confident that use of Mirex at the proposed dosage will result in no excessive or irretrievable impact on the environment.

ACCELERATORS

Sacking the SLAC Way

from a Correspondent

AT a time when the budgets of most physical institutions are declining, at least in real terms, the response of many directors is to reduce their payroll *pro rata*. Job prospects for those dismissed as a result of budget cuts are not always too bright. A notable exception from this law of the jungle is the budgetary and personnel policy being pursued at the Stanford Linear Accelerator Center (SLAC). There the criterion for making budget cuts is to minimize dismissals.

Under the Atomic Energy Commission budget proposed by President Nixon for the coming financial year, the activity at SLAC will have to be reduced by some 10 per cent. The director, Wolfgang K. H. Panofsky, told a recent meeting of the 1,200 laboratory staff that the reduction implied SLAC would have to decrease its in-house effort by the equivalent of 100 man years. But instead of removing that number of people from the payroll, Panofsky proposed programmes which entail the deferral of salary increases for higher paid members of staff, a series of shortened work-weeks and a policy of leaving vacancies unfilled as far as possible. As a result, Panofsky hopes to keep down the number of involuntary dismissals to between 20 and 40.

Panofsky's salary deferral plan will mean that the higher-paid staff members, approximately 40 per cent of the staff, will have their salary increases postponed by between three and six months. The short work-week part of the programme will attempt to develop long but unpaid week-ends for employees so that the laboratory can minimize its operational costs during that period.

SLAC also plans to reduce outside

purchases of goods and services. In addition, the accelerator will be operated at about 5 per cent less than its present activity, even though this seems to be a considerable under-use of such a capital intensive instrument. Approx-

imately 308 hour shifts of beam operation will be cancelled next year. Previous budget cuts had already reduced the number of shifts from a peak of nearly 700 in 1969 to about 550 a year.

Short Notes

Ecological Eclogue

A POEM on an ecological theme has been composed by presidential hopeful Senator Edmund S. Muskie. Published last week in the *New York Times*, the poem delicately evokes by its broken syntax and irregular stress the savage insults wreaked on man and his environment by industrial wastes, as in the aposiopetic lines,

Not now—we don't need
Environmental black lung, we
Need to look again, find new ways
Stop!

The following two verses (given in prose—the form of the verse is evident) illustrate the depth of the poet's concern with problems likely to become an issue in the 1972 elections, together with his prescription for solving them: "To feed the hungry, house the poor, educate the young and care for the ill but don't foul our biosphere. We can—we have the knowledge, the skill, and the treasure—only our dedication—commitment—is in doubt."

Litterateurs in Washington have been nearly unstinting in their praise for the senator's incursion into the arena of Congressional versification hitherto dominated by the former Senator Eugene J. McCarthy. "He has the genius of William McGonagall though without the rhyme" was the verdict of one critic last week. "Senator Muskie's verse may not be as good as the poems of Mao Tse-Tung", said another, "but it is surely superior to the paintings of Hitler or Sir Winston Churchill".

Communication with China

THE sudden breaching of Chinese isolation by the visit of the American ping-pong players last week has caught the National Academy of Sciences unawares. The chairman of its Committee on Scholarly Communication with Mainland China, Dr John M. H. Lindbeck of Columbia University, died in January and a successor has not yet been appointed. The committee was established in 1966 in face of the substantial barriers to communication raised by the events of the cultural revolution. But the committee's efforts to commune with *Academia Sinica*, the Chinese counterpart of the National Academy of Sciences, have been unreciprocated. "We are now in the process of trying to reassure *Academia Sinica* that this is a non-governmental enterprise and that we are ready to

start discussions if they are", staff members of the Academy said last week. "The ping pong ball has been in their court for some while."

NSF Picks Up after AEC

REDUCTION in the Atomic Energy Commission's 1972 budget for physical research will lead to the abandonment of some 140 projects worth a total of about \$10 million. By arrangement with the AEC, the National Science Foundation is informing the scientists in charge of the discarded projects how to solicit the NSF for grants to continue their work. Proposals from abandoned AEC scientists will have to compete on merit with other proposals submitted to the NSF. For this reason it is not known what proportion of the AEC projects will eventually be saved from the scrap heap.

More Trouble for the AEC

THE Atomic Energy Commission last week announced plans to conduct an underground nuclear test of nearly 5 megatons at Amchitka Island in the Aleutians. The test, projected for the autumn, seems likely to bring the AEC into conflict with the environmental lobby, Japan, Canada, Senator Gravel of Alaska, Senator Fulbright and the Senate Foreign Relations Committee and opponents of the Safeguard antiballistic missile system, to name but a few. The test, code-named Cannikin, is said to be for the warhead of a Spartan missile. Senator Humphrey has already called for cancellation of Cannikin, saying such tests are inappropriate during the SALT talks.

Another class of critics are environmentalists and those who fear the explosion will trigger off earthquakes and attendant tsunamis in the region. The Amchitka test site, established by the AEC in 1965 when nobody cared much about these matters, is situated over an active seismic zone. The risks of triggering a major earthquake in the region by a nuclear explosion must be impossible to calculate with certainty; almost the best that can be said is that Milrow, a 1.2 megaton test exploded in 1969, did not cause an earthquake. What Milrow did cause were protests from Canada and Japan and four days of hearings before the Senate Foreign Relations Committee. Cannikin seems certain to be even more troublesome for the AEC.

NEWS AND VIEWS

A Current Trend in Palaeoanthropology

TRENDS exist in many evolutionary lineages and just as these trends are never in straight, undeviating lines so too the trends in scientific thinking tend to travel a winding and sometimes circuitous route. One of the major trends today in palaeoanthropology, the study of fossil man, has been a decline in basically subjective typological reasoning and a move towards the use of more objective, comprehensive and, increasingly, statistical methods. The former method involves a feature by feature examination, often deeply involved with anatomical minutiae but just as often missing the larger morphological complexes; in other words, not seeing the beach for the sand. The latter method recognizes the analytical importance of trait complexes.

Le Gros Clark, one of the first to recognize the importance of the trait complex in this field of study, used the term "total morphological pattern" to describe the most significant morphological features characteristic of each major grade or group of fossil human populations; this approach has recently been increasingly quantified in various types of multivariate analyses. The implicit concept in the total morphological pattern is that the characteristic occurrence of trait syndromes should form the basic unit of study; discrete traits are not important as analytical units. Because of the fragmentary condition of most fossil remains the scope of the trait complex approach is of course limited; in most cases it is confined to the size and morphology of the cranial bones and teeth.

The continuing move away from the trait as the unit of study towards the trait complex has carried with it the necessity of multivariate analyses assisted by computer. The computer, already a basic tool in many fields, is now coming of age in several anthropological disciplines. Stone tools, ceramic potsherds, linguistic components, primate locomotion and even Neanderthal burial patterns are all recent computer fuel. In the field of physical anthropology the study of living groups has long been moving away from typological classifications and forming, in the process, the various mathematical models of population genetics. The statistical concepts of population genetics have much to offer the palaeoanthropologist. Primarily it allows him to consider his sample, however small, as part of a larger population, and to make the population, not the individual, the unit of study. That populations, not individuals, are the evolving units has been suggested by Mayr, Simpson, Dobzhansky and others. It is only with this kind of approach that typological thinking is avoided and a perspective more in line with modern evolutionary biology can be substituted. This wider view of a fossil specimen has perhaps been instrumental in the decline of the phylogenetic "tree" so treasured by an earlier generation of anthropologists. Today it is difficult to imagine *Homo erectus* evolving, as Carleton Coon once did, five different times in five different places into *Homo sapiens*. The current view involving a synthesis of thought from a number of fields visualizes *Homo erectus* as a widespread Old World group, interbreeding, migrating and slowly developing the genetic components of *sapiens* from those of *erectus*.

Although much work needs to be done in quantitatively expressing trait complexes and in describing their taxonomic implications some useful work has already been done in this direction. One of the first studies involving the description and analysis of a fossil hominid specimen using a multivariate technique was made by Weiner and Campbell on the Swanscombe cranial fragments (in *The Swanscombe Skull Report*, Occasional Paper No. 20, Royal Anthropological Institute, London; 1964). These remains, from a Thames River Valley deposit dated to about 250,000 BP, consist of an occiput and both parietals. They were subjected to a D^2 analysis based on seventeen measurements. The D^2 statistic, frequently used on living populations, is theoretically a measure of biological distance and evaluates the amount of morphological divergence between groups. Although Swanscombe had long been considered closely affiliated with modern *Homo sapiens* the multivariate analysis indicated a closer resemblance to the neanderthaloids. Other recent work has involved the quantification of the functional capabilities of a fossil specimen, and it is to be expected that clearer data about the functional nature of trait complexes will be of considerable importance in the definition of valid and useful taxonomic criteria.

As stated earlier, trends, both philosophical and evolutionary, seldom demonstrate strict orthogenetic tendencies. Although the trend in fossil man studies is clearly toward trait complex multivariate techniques, acceptance of this approach has been neither overwhelming nor entirely satisfactory. Admittedly some useful work has been done, but as with new methods in any field, problems are becoming apparent. In some cases statistical methods and theory have been accepted and applied too uncritically by workers schooled primarily in other disciplines. Also a curious sort of *deja vu* occasionally appears when a worker applies statistical methods to univariate or typological data.

Obviously all fossil material cannot usefully be submitted to a comprehensive statistical analysis; very fragmentary, severely eroded or pathological specimens should obviously be excluded. But most material can reveal more useful information through a multivariate examination than through more cursory methods. The "*Telanthropus*" remains discussed recently by Wolpoff (*Nature*, 230, 398; 1971) are a case in point. The early man site at Swartkrans in South Africa had yielded only specimens of the robust type of australopithecine until the smaller hominid referred to as "*Telanthropus*" was found in the late 1940s. Although this taxon is now sunk, a widely accepted nomen has not replaced it. Modern man is highly polymorphic and there is every evidence that early man is at least as variable. Until reasonable levels of statistical confidence can be established regarding expected ranges of variability in early man, taxonomic dilettantism such as that surrounding "*Telanthropus*" will remain meaningless. Possibly the question of the taxonomic affiliation of this hominid and other controversial remains could be removed from the realm of erudite speculation with the more sophisticated analytical techniques increasingly available to the palaeoanthropologist.

MACROMOLECULES

Polyacids Pursued

from our Molecular Biology Correspondent

A DOMINANT attribute of biological macromolecules, in particular the nucleic acids, is their polyelectrolyte character, which in large measure determines their structure and interactions. One of the related problems which has been gnawed at, worried and shaken by any number of physical chemists is the nature of the all-pervading interaction of nucleic acids with magnesium. In a recent study, Sander and Ts'o (*J. Mol. Biol.*, **55**, 1; 1971) have taken advantage of the ion-selective membranes developed in recent years to re-investigate the binding equilibria between magnesium and a series of nucleic acids and synthetic polynucleotides. The binding isotherms are complicated, indicating an effective heterogeneity of binding sites. Sander and Ts'o, making the simplest assumption—that of site binding, that is to say association of each ion with an individual charged site on the macromolecule—express the data in terms of the conventional binding plots (Scatchard equation) modified by an electrostatic interaction term, and extract an apparent binding constant for each species. The strongest binding is found in tRNA—and it will be recalled that the functional “native” state was found by Fresco and his colleagues to be characterized by the tight binding of one or two magnesium ions—followed by various synthetic polynucleotides in double-helical and single-stranded form and DNA and denatured DNA in no very revealing order. The strong binding and high interaction between sites in tRNA is surmised to be related to a compact, globular type of structure, which is not present in the other species examined.

The binding of magnesium to ribosomes, which is of course a prerequisite for structural integrity and biological function, has been studied by Walters and van Os (*Biopolymers*, **10**, 11; 1971). Binding plots for yeast ribosomes showed prominent inflexions, which were present no matter what the concentration of sodium ions. The data can be fitted in terms of theory for the interaction of charged sites spread over a spherical body. The theory indicates that there should be little effect of subunit dissociation, which occurs within the range of the binding curves, on the electrostatic free energy, and this dissociation can therefore be disregarded. The interaction is again treated as site binding, and the stronger and weaker sets of sites are found to be in the ratio of 1.5:1.

The counter-ion layer that forms around a polyelectrolyte is strikingly reflected in the low-angle X-ray scatter-

ing envelopes from DNA solutions, determined by Bram and Beeman (*J. Mol. Biol.*, **55**, 311; 1971). The results can be used to determine the apparent axial radius of gyration, and plots relating this parameter and the scattering angle show two limbs, reflecting the existence of two distinguishable radii of gyration. One of these corresponds to a mass: length ratio corresponding with remarkable precision to the expected value for the B-form of DNA.

Of greater novelty, however, is the second radius of gyration, which implies that the molecule can be represented as two concentric cylinders, the inner one of very high electron density. The outer shell is then surmised to consist of counterions, and possibly, Bram and Beeman suggest, water compressed by electrostriction. When the supporting electrolyte is sodium chloride the counterion shell is 20 Å thick, and it increases as expected when the larger caesium counterions are substituted. The results of earlier workers, for a number of possible reasons, which are analysed by Bram and Beeman, failed to resolve this outer shell. The counterions are thus in the form of a Donnan sheath around the polyelectrolyte, and there is, as expected, no evidence of site binding. It would be of no little interest to examine DNA in the presence of magnesium ions.

A study of counterion interactions with a simple polyelectrolyte, polyvinylsulphonic acid, though without any direct biological orientation, deserves wide attention, both for its aesthetic value and the new directions that it may suggest to the physical biochemist. Taha and Morawetz (*J. Amer.*

Chem. Soc., **93**, 829; 1971) have studied the fluorescence of uranyl ions as a function of the concentration of polyelectrolyte in the solution. The cations congregate in the domain of the polyacid and the fluorescence drops in consequence of self-quenching. In a pH range where the UO_2^{3+} ion is in equilibrium with polynuclear complexes ($\text{U}_3\text{O}_{10}^{2+}$ and higher), rather different spectral changes occur in consequence of the preferential concentration of the mononuclear complex around the polyacid.

When a new quenching species, the ferrous ion, is introduced some striking effects are observed. At low ferrous ion concentration there is a dramatic drop in fluorescence, altogether different from the smooth progressive decline that is observed in the absence of a polyelectrolyte. This results from the confluence of the two anion species in the polyacid domain. Addition of more ferrous ions causes the displacement of the uranyl from the polyacid, and the fluorescence again rises to a maximum, after which quenching in the free solution supervenes and causes it again to fall. As Taha and Morawetz now point out, it is readily shown that such a profile of fluorescent intensity against quencher concentration can only be obtained if there are bound and unbound fractions of uranyl ions, which make separate contributions, and is incompatible with rapid exchange of counterions within the microsecond or so lifetime of the excited state of the uranyl. Thus, as expected, the sticking time of a counterion, which is treated in terms of site-binding, is much longer than this.

Transformations and Rotations

ATWATER recently considered the relativistic treatments which are possible for rotating bodies and came to the conclusion that the application of special relativity could lead to impossible conclusions (*Nature*, **228**, 272; 1970). In next week's *Nature Physical Science*, Marsh and Noonan separately take issue with him about the way in which he applies the Lorentz transformations to his example of a rotating disk.

It was pointed out by Atwater that a pattern of stripes painted on the edges of alternate octants of a transparent disk—which is then rotated at a high speed—would be Lorentz contracted in such a way that the stripes appear not to be uniformly distributed around the disk circumference. This is plainly unacceptable because of the inherent symmetry of a disk. Marsh suggests that it is anyway impossible to make use of the Lorentz transformation in the case of the periphery of a rotat-

ing disk because the velocities of different points on the perimeter are not collinear. He also argues that, quite apart from kinematic arguments, the problems of the behaviour of rotating solids at speeds near to the speed of light have to be resolved before any definite conclusions can be reached about transformations involving rotating coordinate frames.

The possible effects of the stress created in a rotating solid are also emphasized by Noonan in his suggestion that the peculiarly unsymmetric situation, which, according to Atwater, is a necessary consequence of applying special relativity, could in fact be realized if the material were to crack under stress. Atwater's reply, to Noonan in particular, contains the very reasonable observation that the laws of physics must not depend on the intrinsic properties of materials. He also reemphasizes his original contention that an anomalous situation does exist.

SELENOLOGY

Exchange of Information

from a Correspondent

LIKE the NASA-sponsored First Lunar Science Conference at Houston, Texas, in January 1970, the International Astronomical Union symposium on the Moon at Newcastle upon Tyne on March 22–26 was of outstanding interest but for quite different reasons. At the Houston conference, leading specialists presented preliminary accounts of their work on returned lunar samples. At Newcastle, speakers were by no means restricted to discussions of Apollo samples; and they had spent more time absorbing the early Apollo results.

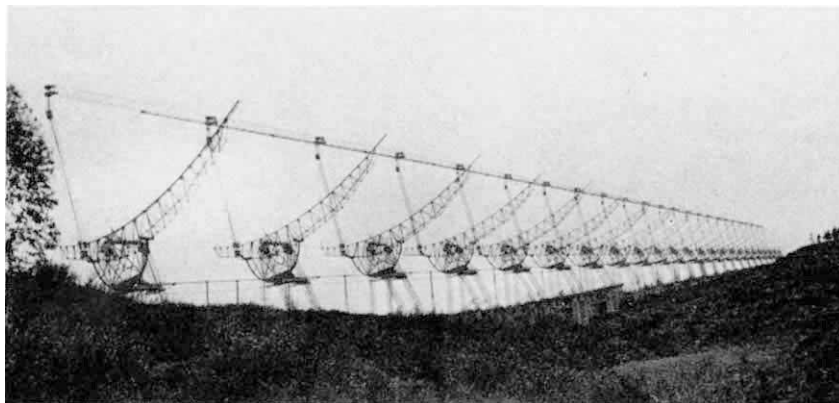
Stressed for their importance, lunar mascons formed a theme that was encountered repeatedly throughout the symposium. Everybody seemed to agree that once the nature and origin of these mass concentrations were settled, they would have a much better idea as to how the Moon originated. Mascons are important not because they are exceptionally large positive gravity anomalies—they are not—but because they exist at all in a Moon that has been thrice dated as of such an age that excess masses would normally be expected to have attained complete isostatic adjustment. Professor H. Urey (University of California, San Diego) maintained that the mascons were very old (although this was not demonstrated explicitly) and that therefore the Moon must always have retained great rigidity and strength. Dr P. Müller (Jet Propulsion Laboratory, Pasadena) had calculated that the stress required to support the larger lunar mascons was in the range of 50 to 400 bars and that a lunar crust, of the order of 100 km thick, could not have been heated to above 800° C at its base but must have been endowed with great strength for geological periods of time. On the other hand, it is relevant to note that there are no age determinations of lunar rocks at any mascon site; and that most observers agree that lavas from hot materials at depth may have flowed more recently than 4.5×10^9 years ago, layer upon layer, at least across those of the maria over which mascons are observed. Lunar absolute altitude determinations indicate that, apparently unlike the mascons, the maria and highlands are, broadly speaking, in complete isostatic adjustment. An important step forward in understanding mascons was announced by Dr Müller: in the case of the Mare Orientale and Mare Autumni mascons, at least, the previously questioned negative gravity anomalies coinciding with the surrounding rings of mountains (Rook and Cordillera, respectively) were said to be a reality—they were definitely not in-

troduced spuriously by the use of simplified methods of reduction.

It seemed that the geologists and geophysicists had their feet on the ground more firmly than did the astronomers when the perennial question of the Moon's origin came under examination. Among others, Professor A. E. Ringwood (Australian National University, Canberra) and Professor M. Brown (University of Durham) emphasized how the extreme diversity of types among the lunar basalts argued for partial melting processes at depth in the Moon.

By comparison with the relatively concrete (albeit based on poor sampling) starting point of the mineralogist and petrologist, the classical astronomer's approach to the problem of the Moon's origin seemed particularly weakened. Mr L. V. Morrison (Royal Greenwich Observatory, Herstmonceux)

reported that a value of the secular acceleration of the Moon deduced from recent occultations tended to agree closer with the value deduced from ancient eclipses than with the value of about twice as much (minus 22 seconds of arc per century) which Spencer Jones deduced from observations of the longitudes of the Sun, Moon and planets. It is well known that the whole question of the recession of the Moon from the Earth depends on such unknown factors as the past elasticity of the Earth as a planet. The assumptions that are made by planetary dynamicists regarding the state of the Earth's interior in the geological past are particularly difficult to investigate. This is one of the many examples, given implicitly at this symposium, of the need for communication and understanding between scientists studying one and the same problem from diametrically opposed schools.

India Enters Radio Astronomy

THIS illustration comes from an article in next Monday's *Nature Physical Science* in which G. Swarup and his colleagues of the Tata Institute of Fundamental Research, Bombay, describe the new radio telescope at Ootacamund, in the Nilgiri Hills of south India. Basically, the frame of the telescope consists of twenty-four parabolic frames spaced 23 m apart, and the reflecting surface is made up of 1,100 stainless steel wires stretched between the two end frames and supported at each intermediate frame. They give the telescope an effective collecting area of 8,700 m², equivalent to a parabolic dish 138 m in diameter operating with an aperture efficiency of 60 per cent. By installing the telescope on a hill, the slope of which is the same as local latitude, 11° 23', the axis is made parallel to the rotational axis of the Earth, and radio sources can be tracked in hour angle for 9½ hours per day.

Swarup and his colleagues report some of the results since the first observations with the telescope in February last year. At present the telescope is tuned to 326.5 MHz, with

a band-width of 4 MHz, but eventually the focus will also be fitted with dipoles for observations at 110 MHz. So far the radio astronomers in India have been particularly concerned with the determination of accurate positions for radio sources using the lunar occultation technique, and Swarup and his colleagues explain how the new telescope is well suited for work of this sort.

They report that the occultations of 150 sources have been recorded, in most cases leading to positions of 1 s of arc or better in both right ascension and declination. In forty-one cases there has been an attempt to find the sources on the Palomar Sky Survey prints, leading to thirteen successful identifications. The telescope is also to be used for work on pulsars—the Indian team hope to find more weak pulsars—and on interplanetary scintillations. And two radio dishes, 13.5 m in diameter, are being set up nearby to form an interferometer. The telescope was built entirely in India, and Swarup and his colleagues are hoping that it will be a valuable impetus to astronomy in India.

GEOPHYSICS

European Colloquium

from a Correspondent

ONE of the aims of the First European Earth and Planetary Physics Colloquium held at the University of Reading from March 29 to April 2 was to attract research students and others who find it difficult to obtain funds for long distance travel to attend scientific meetings. As it turned out, more than a third of the four hundred or so participants at Reading were younger scientists. The meeting was also successful in attracting Earth scientists from all over Europe; all Western European countries except Finland and Luxembourg were represented, and large groups attended from West Germany (46), France (31), Italy (27) and Holland (16).

The scientific programme was divided into six interdisciplinary symposia with invited speakers and three "open" sessions for submitted contributions. The importance of an interdisciplinary approach to many of the major problems of geophysics was clearly demonstrated. It was shown, for example, how studies of the dynamics of rotating fluids are applied to motions both of the Earth's liquid core and of planetary atmospheres; how physical chemistry and chemical petrology can contribute fundamental data on the composition of planetary atmospheres; and how petrological and geophysical approaches play complementary parts in the study of the physical properties of the Earth's mantle. Participants migrated freely among parallel sessions. Three successful "frontier" lectures were given in the evenings outside the normal programme, on topics in which major advances are being made.

Future policy was discussed at two open business meetings. Some senior scientists expressed reservations about the desirability of establishing a distinct European organization for geophysics, but most of the participants clearly felt this to be the simplest way in which regular European meetings, accessible to younger scientists, could be assured. Accordingly, it was formally resolved to establish an Association of European Geophysicists the primary purpose of which would be the organization, annually if possible, of the European Earth and Planetary Physics Colloquia. The programme of future meetings would broadly follow the pattern established at Reading, except that the "open" scientific sessions would be subdivided more along the lines found successful by the American Geophysical Union and the International Union of Geodesy and Geophysics, in order to ensure full participation by geochemists and others whose interests overlap conventional geophysics; it was felt that the

rather idealistic subdivision of these sessions at the Reading meeting into planetary interiors, planetary atmospheres and interplanetary processes may have deterred many geologists and geochemists.

A broadly based steering committee was set up with the task of finding national representatives to facilitate communications, and to ensure that other organizations, including the European Association of Exploration Geophysicists, were kept fully informed of the plans of the new association. Representatives of the American Geophysical Union and the Royal Astronomical Society undertook to distribute information through the newsletters of their organizations.

As a regional geophysical meeting the Reading colloquium was an undoubted success. The proposals for future meetings of its kind deserved support of everybody with an interest in the dissemination of ideas among geophysicists and a concern for the training of European Earth scientists.

BRAIN FUNCTION

Chemistry of Learning

from a Correspondent

TEN years ago, any meeting with behaviour in its title would have been unlikely to have a biochemist among its participants. But at a recent symposium on macromolecules and behaviour at the University of Birmingham on March 18 and 19, at least 50 per cent of the contributors were biochemists—a real indication that the biochemist is becoming more involved in the psychologist's problems, especially in the field of learning and memory.

The first session was concerned with neurochemical correlates of behaviour with particular reference to learning. Opening the session, Professor H. Hydén (University of Gothenburg) described some sophisticated techniques for measuring the uptake of labelled amino-acids into individual neurones and glia from discrete parts of brain. Rats were taught to use their unpreferred paw, during which training

Hamster Sarcoma Virus Identified

IN next Wednesday's *Nature New Biology*, Nowinski, Old, O'Donnell and Kingsley Sanders provide evidence that hamsters have their own antigenically distinct group of sarcoma and leukaemia viruses: chickens, mice, cats and some other species are already known to have their own private RNA tumour viruses (oncornaviruses). The hamster virus (HaSV) studied by Nowinski *et al.* is released into the medium by a tissue cultured line of hamster sarcoma cells established by Bassin *et al.* (*Intern. J. Cancer*, **3**, 265; 1968). The sarcoma from which the cells were derived appeared in a newborn hamster following the injection of mouse cells infected with a murine sarcoma virus. The hamster sarcoma virus, however, does not infect mouse cells but, as has been shown by Zavada and Macpherson (*Nature* **225**, 24; 1970), is very efficient in transforming the BHK21 hamster cell line, *in vitro*. The origin of the HaSV is therefore not clear; it may be a modification of the murine sarcoma virus or it may be a hamster virus stimulated by the inoculation of the infected mouse cells.

Nowinski *et al.* prepared rat antiserum against one of the major internal structural proteins (the group specific or gs antigen) of HaSV and showed that it reacted in immunodiffusion tests with hamster oncornavirus gs antigens, but not with comparable antigens derived from murine, feline, and avian oncornaviruses. These gs antigens are of general biological interest because they are present in the tissues of some apparently normal inbred chicken and

mouse embryos. Moreover, Payne and Chubb (*J. Gen. Virol.*, **3**, 379; 1968) have shown that the expression of the antigen in chicken embryos is regulated by a dominant gene. Huebner *et al.* (*Proc. US Nat. Acad. Sci.*, **67**, 366; 1970) have shown that an analogous situation occurs in mouse embryos. Normal adult tissues do not contain detectable amounts of the gs antigen found in their indigenous oncornaviruses and immunologically mature animals are incapable of producing antibodies against this antigen—a fact that suggests these animals have had previous experience of the antigen in embryonic life and may continue to receive antigenic stimuli during adult life.

It is difficult to reconcile all these facts and decide whether the gs antigen is coded by cell or viral genes or both. Are the antigens normal cell components that have a role in certain stages of embryogenesis and which in oncornavirus infections are produced and incorporated into the virus particle? Or are, as has been suggested by Huebner, the cell genes the DNA proviruses of that particular species' oncornaviruses? It seems very likely that the virus particles carry the genetic information for their own gs antigens, because some chicken sarcoma viruses transform rodent cells and, although these cells may not produce infectious virus, they usually contain chicken gs antigen. An alternative but less likely explanation of this result is that particular strains of oncornaviruses always elicit the same antigen even in the cells of different species.

there was an initial stimulation in the uptake of ^3H leucine into the hippocampal region. Hyden described how, as training proceeds, this enhanced uptake shifts to the cerebral cortex, while labelling in the hippocampus returns to normal. His study of S13, S14 and S100 proteins has also shown that the S100 protein which, before learning, is isolated as one band on electrophoresis, is recovered as two bands after a learning experience.

Dr D. A. Booth (University of Sussex) discussed the effects of cyclohexamide, an inhibitor of protein synthesis, on memory consolidation. He argued that it was still open to question whether the disruption of long term memory by cyclohexamide was the result of the blocking of the consolidation process or was the result of negative reinforcement arising from aversive characteristics of the drug. Dr J. T. Rick (University of Birmingham) pointed out that the majority of studies concerned with biochemical correlates of learning make use of aversive conditioning stimuli in order to obtain rapid overlearning on the part of the animal. Stimulation, such as electric shock, is known to have significant effects on brain chemistry independent of any learning process. He argued that studies based on the extinction rather than the acquisition of a learned response, when such stimulation is absent, might overcome this difficulty.

The simpler nervous systems of invertebrates have been well exploited in this area of research as exemplified by the work described by Professor G. A. Kerkut and Dr G. W. O. Oliver (University of Southampton). Using the common garden snail (*Helix aspersa*) and the cockroach (*Periplaneta americana*) these workers described how, during learning, acetylcholinesterase activity in the nervous system increases in the former where the transmitter acetylcholine is chiefly inhibitory and decreases in the latter where it is only excitatory. Professor Kerkut went on to argue that one explanation of these data is that changes in interneuronal activity that must accompany learning may be affected by conformational changes in the enzyme rather than by differential rates of its turnover. The state of the enzyme, in other words, would act like a switch increasing or decreasing synaptic security.

The second session was concerned with modifications of brain chemistry in relation to behaviour. Dr J. Dobbing (University of Manchester) reviewed some of his studies on the effects of various early environmental conditions on future brain development and the extent to which such effects depend on just when these conditions occur relative to the normal time scale

of maturation for the species. Dr G. Ungar (Texas Medical Center, Houston) dealt with the possibility of "learning" as a transferable phenomenon. He described the amino-acid sequence of a polypeptide extracted from the brain of a trained animal which, after injection, specifically aided the learning of a naive animal. Professor P. Mandel (Centre de Neurochimie, Strasbourg) was critical of such possibilities, and took the idea to the logical conclusion as he perceived it; that is, because the synthesis of RNA is only possible where the complementary DNA is already established, every unit of acquired information must already have a receptor-site in the brain.

Macromolecular Stereopathology

AN article by Greer and Perutz in next Wednesday's *New Biology* may suggest to the imaginative mind the birth of a new branch of clinical science—diagnosis by crystallography. The number of recognized human haemoglobin variants now runs well into three figures. Most are only structurally, and not functionally, distinguishable from the normal molecule, and do not give rise to clinical symptoms. Others do their possessors no good at all: the most famous, sickle-cell haemoglobin, has an anomalous tendency to aggregate in the deoxygenated state, and comes out of solution with disastrous consequences for the red cell. Some others are aberrant in their oxygen equilibria: one, Hb-H, is generated by an imbalance in the concentrations of α - and β -chains, and is a tetramer made up of four normal β -chains, which are incapable of generating haem-haem interactions, and the contingent sigmoidal oxygen uptake curves. In a further set of variants an amino-acid substitution in a position intimately related to the binding of the haem to protein leads to other types of functional failure, in some cases (the haemoglobin-M variants) the complete inability of the associated haem to bind oxygen.

The species that Greer and Perutz have now examined is haemoglobin Rainier, which is a β -chain mutant with its substitution at the important, C-terminal end, where there is strong sequence conservation between all vertebrate haemoglobins. The conversion of tyrosine-145 to cysteine brings with it a substantially decreased haem-haem affinity and Bohr effect, and an increase in oxygen affinity. The characteristic disorientation of the crystal lattice of the oxyhaemoglobin on deoxygenation was observed in haemoglobin Rainier crystals, which indicates that its abnormality does not arise from any inability of the quaternary structure to change from the

REVERSE TRANSCRIPTASE

Transforms Initiated

from our Cell Biology Correspondent

THE extent to which research groups are falling over each other in the headling rush to get into the reverse transcriptase scene is reaching ludicrous proportions. In the circumstances, repetitious research is inevitable, but repetitious publication is not. Isn't it time for some sort of moratorium? For example, it might be agreed that after a first token publication—the claim to a stake—further publications should await the collection of more than the odd datum. But, of course, there is

oxy-pattern to the deoxy-pattern. A comparison of deoxygenated haemoglobin Rainier with the normal adult protein showed a clean difference Fourier map, except in the region of the β -chain C-termini. The missing tyrosine is apparent as a negative peak of electron density, and the cysteine sulphur shows up as a strong positive feature. Moreover, the normal cysteine-93, which normally resides in the F-helix, on the other side from the haem-liganded histidine, is rotated about the α - β carbon bond, so as to move the sulphhydryl towards the pocket that normally contains tyrosine-145. The alignment of the normal with the abnormal cysteine side chains signifies the formation of a disulphide bond between them. This event causes a local conformational convulsion, in which the α -carboxyl group and the imidazole of the terminal histidine-146 are displaced, so as to break the ion-pairs in which they are normally involved, and the residue is drawn into the α -helix, in which it is found in myoglobin. Perutz and his co-workers previously demonstrated that one of the broken ion pairs is responsible under normal conditions for the alkaline Bohr effect.

Neither can the normal conformational change on deoxygenation described by Perutz, which involves the displacement of the tyr-145 side chain from its pocket, take its course. Moreover, model building suggests that in haemoglobin Rainier the α -carboxyl group can form an adventitious ion-pair with the α -amino group of the partner β -chain to provide a further source of stabilization of the oxy-form. The disulphide bond also presumably stabilizes the β -chain conformation, which may be the cause of the anomalous stability of haemoglobin Rainier towards alkaline denaturation. A "molecular disease" then, specified beyond Pauling's most optimistic predictions of more than twenty years ago.

always one thing to be said for a morass of mediocrity; it throws into sharpest relief anything which rises above it. The experiments reported by the Hanafusas in a short communication to *Virology* (43, 313; 1971) are very much a case in point. Instead of the minor variations on the themes of templates, inhibitors and distributions of reverse transcriptases these workers, by exploiting a variant Rous sarcoma virus (RSV(O)) they isolated in 1968, have provided the only direct evidence that this enzyme is required for the establishment of transformation of chick cells by RSV. And into the bargain their results suggest that the reverse transcriptase in RSV particles is not a preexisting host enzyme which is incorporated into virions as they mature.

The variant virus was isolated from chick cells transformed by the Bryan strain of RSV. Of the cells transformed by this virus 90 per cent liberated infectious progeny sarcoma virus, designated RSV β (O), but 10 per cent of the transformants yielded physically normal progeny, designated RSV α (O), which are noninfectious and cannot transform susceptible cells. That this lack of infectivity is not simply the result of the failure of RSV α (O) particles to penetrate cells was shown by fusing the virus into its hosts with inactivated Sendai virus; the cells were not transformed. But after the 10 per cent of transformants which yield RSV α (O) are superinfected with an avian leucosis virus, about three-quarters of the cells start yielding pseudotypes, RSV α genomes enclosed in envelopes containing leucosis virus antigens (RSV α (ALV)), which are infectious and transform. But the progeny viruses produced by a cell transformed by an RSV α (ALV) pseudotype again are only noninfectious RSV α (O) particles.

Why are RSV α (O) particles noninfectious and unable to transform? One obvious possibility is that they lack reverse transcriptase. If this were the case, and if the virus could not use any putative reverse transcriptase indigenous to its host, double-stranded DNA proviruses of RSV α genomes would never be made, and the virus would not be infectious. Sure enough, when the Hanafusas assayed RSV α (O) for reverse transcriptase activity they failed to detect it. By contrast, the infectious pseudotypes, RSV α (ALV), possesses this activity. The obvious conclusion is that RSV α (O) is noninfectious because it lacks this enzyme and cannot make a provirus. Either the RSV α (O) particles contain a functional enzyme but also a potent inhibitor of its action or they carry a defective enzyme presumably because the gene which specifies it is mutated. It is at present

difficult to distinguish between these two explanations. The observation, that when RSV α (O) particles are mixed with avian leucosis virions and then assayed, the detectable activity is less than that with the same amount of leucosis virus alone, is compatible with both alternatives. For defective enzyme, as the Hanafusas point out, might competitively inhibit an active enzyme.

The finding that the RSV α (ALV) pseudotypes possess transcriptase activity and are infectious suggests that these particles pick up active enzyme specified by the superinfecting leucosis virus. And the observation that cells transformed by RSV α (ALV) pseudotypes are indistinguishable from cells transformed by infectious RSV β (O) indicates that reverse transcriptase plays a part in the establishment but not in the maintenance of transformation. Finally, a cell superinfected by leucosis virus produces in addition to pseudotypes, RSV α (ALV), some straight noninfectious RSV α (O). The former virus has transcriptase activity, the latter does not. This suggests perhaps that the reverse transcriptase in these viruses is not a pre-existing cellular enzyme.

SODIUM CHLORIDE

More Control Systems

from our Microbiology Correspondent

It has been shown recently that additions of sodium chloride to growing bacterial cultures cause major changes in the free amino-acid pools and especially in the concentrations of glutamate (Tempest, Meers and Brown, *J. Gen. Microbiol.*, 64, 171; 1970). The free amino-acid pools of a range of Gram positive and Gram negative bacteria were significantly different when the organisms were grown on identical media and when all other environmental parameters were constant. The predominant amino-acid, glutamic acid, accounted for 52 to 89 per cent of the free pools in ammonia-limited cultures. Increasing the growth rate seven-fold induced a predictable rise in the concentration of free amino-acids from 5.5 to 14.7 mM, but these changes were small compared with those resulting from the additions of salt to the cultures. For example, 4 per cent NaCl caused a nine-fold increase in the free amino-acid pool

Nature of the M87 X-Ray Source

ALTHOUGH the orbiting Explorer 42 satellite is beginning to oust rocket and balloon borne X-ray experiments from their position of prime importance, the group of astronomers at the University of Leicester, who have been among the most successful X-ray rocketeers, are still producing valuable data, and with the aid of a colleague from the Institute of Theoretical Astronomy at Cambridge are carrying out the analysis without which observations are of very little significance. Their latest report, in the forthcoming issue of *Nature Physical Science*, concerns another flight by the apparently infallible Skylark rocket which has done so much to open up this new field of high energy astrophysics.

A. F. James, K. A. Pounds, M. J. Ricketts and M. J. Rees have studied the data obtained from a flight carried out primarily to study the isotropy of the diffused background of X-radiation which seems to permeate space but, as the rocket scanned the X-ray sky, one of its detectors viewed M87 and found an X-ray flux at the 5σ significance level in the position of this source. The other detector, for which the line of sight passed rather below M87, did not detect any corresponding peak in the X-ray spectrum, and this ties the position of the source to that of the external galaxy fairly accurately. The evidence shows, however, that both the intensity and slope of the X-ray spectrum of this object has changed over the two years

since it was first determined by several groups in 1968.

The variation of celestial objects over short periods of time is an important tool in determining their sizes. A source can only vary coherently in a time less than that taken for light to cross its shortest width—otherwise, variations would occur out of step and cancel each other out. This test is most familiar from its application to quasars. In the case of M87 it seems that the central region of intense X-ray activity must be smaller than one light year. This could mean that the source is concentrated in the nucleus of M87, or it may be in the jet (some 10 light years long) being emitted from this giant elliptical galaxy.

Partly from the evidence of radio observations of the jet and galaxy, James *et al.* favour the alternative that the energy source is in the galaxy nucleus. It is unlikely (although not impossible) that this compact source is radiating by synchrotron emission and so a choice must be made between an inverse Compton mechanism in which low energy photons are increased in frequency as they gain energy in a very active nucleus (for example, scattering of infrared photons by energetic electrons), and a thermal bremsstrahlung mechanism in which the gravitational energy of matter falling on to a very massive point mass or black hole is converted into heat and thus produces intense radiation at all frequencies.

when *Aerobacter aerogenes* was growing at a rate of 0.1 h^{-1} and an even more pronounced (seventeen-fold) increase when the growth rate was increased to 0.3 h^{-1} . These changes in pool size resulted very largely from disproportionate increases in glutamate. The response of *A. aerogenes* to salt was rapid and reflected a *de novo* synthesis of glutamate. Although other Gram negative bacteria showed a similar rapid response to NaCl, Gram positive species showed an enhancement of the free amino-acid pool only after prolonged exposure to NaCl.

The full interpretation of these data remains to be made but some fascinating control systems are indicated. The rapid change in glutamate concentrations in Gram negative bacteria is most likely caused by the activity of altered glutamate dehydrogenase. In *Aerobacter*, Tempest and his colleagues found that this enzyme showed marked pH dependence and, arguing that addition of salt would cause a rapid efflux of protons and hence an increase in the intracellular pH, they suggest that this is the most feasible explanation for the rapid increase in glutamate. Subsequently the intracellular proton concentration would be restored and glutamate synthesis established at a new steady state rate. Whereas the maintenance of a constant proton concentration in Gram negative bacteria may control the activity of glutamate dehydrogenase and hence the level of free glutamate, Tempest and his colleagues (*ibid.*, 187) have found that similar reasoning does not explain the notable difference in glutamate concentrations of Gram negative and Gram positive type.

The amino-acid pools of the yeast *Saccharomyces cerevisiae* were not affected either quantitatively or qualitatively by sodium chloride; rather the supply of ammonium ions was the controlling factor. Nevertheless, the recent work of Watson (*ibid.*, 91) indicates at least one important way in which salt influences yeast growth. Using salt concentrations higher than those used by Tempest *et al.*, Watson has shown that the maximum specific growth rate of yeast falls when sodium chloride is added to the growth medium. Studies of steady state cultures enabled Watson to conclude that the reduced growth rate is caused by an increased requirement for glucose, the energy-yielding substrate, and that this is related to a higher demand for maintenance energy. Watson's data are important in providing a clear demonstration of one of the functions of maintenance energy in micro-organisms; the increased requirement could be related to the maintenance of an intracellular concentration of sodium ten times lower than that of the extracellular medium.

EARTH SCIENCE

Free Oscillations

from our Geophysics Correspondent

A RECENT article by V. S. Tuman (*Nature*, **229**, 618; 1971) looks set fair to cause a major controversy in geophysics. First reports suggest that there are some geophysicists who are impressed with a spectacular achievement alongside those who believe the interpretation of the data to be utterly erroneous.

For many years, seismologists have hoped for a seismometer sufficiently quiet at long periods (up to one hour) to permit continuous spectral analysis of long stretches of record revealing the Earth's free oscillations. The Earth under the influence of some disturbance like an earthquake will be set into vibration in an infinite number of modes. The amplitude distribution of surface motion in such modes is described by spherical harmonics (although the pole of rotation will not coincide with the coordinate pole for the harmonics). Interaction between

the free oscillations and the Earth's rotation removes degeneracy in spherical harmonics of the same degree, as does a non-spherically symmetric distribution of elastic constants and density. These effects are well documented theoretically and the splitting of spectral lines by rotation is well documented observationally. In addition to fundamental modes, there are a whole realm of overtones corresponding to spherical nodal surfaces within the Earth. Free oscillations of the Earth have a very close parallel in many quantum mechanical phenomena.

Calculation of the eigenfrequencies of standard Earth models is time consuming but not overwhelmingly difficult. Splitting of modes, for whatever cause, is also calculable. The gravest mode has a period of 53 minutes. Higher degree harmonics have shorter eigenperiods—the fortieth degree harmonic has a period of three and a half minutes. Overtones increase the density of eigenfrequencies at the shorter period end and splitting produces discernibly separate spectral peaks

Antigens and the Lymphoid System of Chimaeras

It is now a quarter of a century since Owen's realization that some non-identical twin pairs in cattle were chimaeras in the sense that each contained some red blood cells which had the antigenic phenotype of their twin. The discovery of this phenomenon influenced the development of the theory of immunological tolerance as a means by which an organism could learn not to react against antigenic components of itself. In the cattle twins it was supposed that in the early stages of foetal development the condition of synchory (placental fusion, essentially) had allowed the exchange of precursor cells between the twin foetuses and that these cells had been tolerated and accepted as self. But, aside from this corollary, the recognition of the chimaeric state established the principle that two different cell lines could grow in the same host animal and their products could be recognized and quantitated.

In 1956 it became clear that it was possible to produce chimaeras artificially when irradiated animals were injected with haematopoietic cells. This further discovery intensified the search for means of analysis of chimaeric conditions by the development of methods by which somatic cells could be recognized by other criteria than simple morphology. A variety of genetic, biochemical, functional and antigenic markers has emerged, the utilization of which has become a major pastime with cellular immunologists. Many experiments consist of the identification of cells in particular

organs or responding to antigenic stimulation in chimaeric animals. In next Wednesday's *Nature New Biology*, Miller and Sprent comment on the use of a popular marker of thymus derived cells, the theta (θ) antigen.

Most cells in the thymus are strongly θ positive and it has reasonably been assumed the lymphocytes external to the thymus which are θ positive are of thymic origin—T cells. On the basis of this assumption Raff and his colleagues (*Nature*, **224**, 378; 1969) have determined the numbers of T cells in the various lymphoid organs of a mouse. This analysis was not performed on chimaeras. Miller and Sprent have concentrated their attention on cells in the thoracic duct lymph of chimaeric mice and, like Raff and Wortis (*Immunology*, **18**, 931; 1970), have concluded that they are mostly T cells. Miller and Sprent, however, took the matter further. They used an additional iso-antigenic marker on transferred T cell populations and found a discrepancy between the numbers of cells scored as T cells in the secondary hosts, on the basis of θ antigenicity, and the numbers estimated by lysis with an isoantibody. Their conclusion is that at least some T cells are efficient in θ antigenicity.

Some caution must therefore be exercised in experiments which are anti- θ antisera either as a means of quantitation or elimination of T cells. But, in spite of this setback, it is to be expected that the use of tissue specific surface antigens to analyse the structure of the lymphoid system is here to stay.

at low degrees, merely broadened lines at higher degrees. There is no formal distinction between Rayleigh waves and Love waves on a spherical surface and free oscillations. For instance, the very long period "spheroidal" oscillations can be regarded alternatively as very long wavelength (a fraction of the Earth's circumference) Rayleigh waves.

Free oscillations of the Earth are obviously excited by the slightest tap but instrumental noise levels, the inherent absorption in the Earth and the random occurrence of small earthquakes have up to the present made any identification of spectral lines in the noise background impossible. The largest earthquakes (in particular: Chile in 1960 and Alaska in 1964) have led to signals sufficient to permit many tens of spectral peaks to be found (and used to refine Earth models). In the past year or two a new design of gravimeter at San Diego has shown free oscillations with a signal-to-noise ratio of up to 20 dB for many earthquakes down to magnitude 6.5 (in which the energy release would be several thousand times less than that for the Alaskan earthquake). And several other institutions report further free oscillations from new instrumentation responsive to ultra long periods and with low inherent noise levels—instruments which can reliably sense movements down to a few Ångströms per hour are being looked for. But getting spectral lines out of the background on a day when no major event occurs, only smaller quakes, is a plum which many seismologists would like to pick, not just for fame but because seismic refinement of the structure of the Earth could then be a continuous process, not needing major earthquakes. Has Tuman pulled off this coup with his cryogenic gravimeter, certainly the most unusual instrument in geophysics today?

It is, of course, unfair to judge a conclusion that "four independent types of evidence indicate that spheroidal oscillations have been recorded continuously" solely on what is published. On the other hand, it can reasonably be assumed that the article puts as good a case as the author can make. On this basis, the assertions seem quite unjustifiable. Spectral analysis of 72 hours of random noise would produce a jagged output with a known mean and variance. There will be apparent spikes in the spectrum, but one can draw confidence limits around the mean and assert that these limits will not be exceeded more than so many per cent of the time. The statement that the signal ranges from 2 to 9 dB above the mean is unimpressive when random noise would produce similar fortuitous peaks, and a claim that several of the peaks are over the 95 per cent confidence limit invites the retort "yes, it

looks like 5 are". Nothing in the data published suggests any deviation from a stochastic process. As has been noted, the density of spectral peaks of free oscillations increases rapidly towards the short period end, and this means that it will always be possible to pick an appropriate degree, order and overtone to fit any spectral peak. Thus simple association of peaks with modes does nothing to verify their reality with such poor signal-to-noise ratio. At longer periods the density of modes declines as does the density of sample points from a finite data window so the fit of peaks to modes should fall in quality, which indeed it does. There seems, however, to be just as many unexplained peaks at 800 seconds as there are at 400 seconds although the latter are called possible unknown overtones whereas the former are not mentioned. Does the spectrum continue in its jagged way up to the gravest mode and beyond, where if this were real free oscillation signal it should diminish to instrumental noise levels?

No data are shown. Equally curious is the identification of the gravest mode by its absence. This is claimed to be a result of the Earth's rotation in conjunction with the geographic location of the instrument. Because the small earthquakes assumed to be sustaining the free oscillations are presumably randomly located, the geographical location of the instrument could be assumed random and the earthquakes fixed in position, in which case there is no reason at all to expect rotational suppression of the gravest mode.

The least Tuman can be expected to do now is to show some of his seismograms. If the instrument really is sensing ground movement and its sensitivity is all that is claimed, the Earth tides should be a colossal feature, yet they are not mentioned.

It is decidedly distasteful to pick a public argument with a colleague, but Tuman's claims far exceed his published data, and the subject is, it has been noted, one in which there is a decidedly ripe plum for the winner.

Seeking Human Cancer Viruses

ALTHOUGH it has so far proved disappointing the search for human cancer viruses goes on apace. The direct approach is obviously to look in human cancer cells and in the body fluids of cancer patients for complete virus particles or for biochemical or immunochemical evidence of viral infection. So far this approach has revealed only two strong candidates: the Epstein-Barr herpesvirus, which is associated with Burkitt's lymphoma, and the B-type virus particles recently detected in some human milks (see *Nature*, **229**, 593, 627; 1971), which in structure closely resemble the virus(es) which can cause breast cancer in mice. There is, however, a more subtle approach to the problem which has been used successfully for the isolation of animal cancer viruses, and in next Wednesday's *Nature New Biology*, McAllister and his six colleagues describe how they have exploited this in an attempt to isolate a human sarcoma virus.

The cat leukaemia and sarcoma viruses are no respectors of species barriers; they infect, and the feline sarcoma virus transforms, cells from a very wide range of species including man. Furthermore, the feline leukaemia virus is able to act as a so-called helper and rescuer of unrelated tumour viruses. For example, after infecting with feline leukaemia virus hamster cells transformed by a mouse sarcoma virus, progeny sarcoma virus is liberated, even though, before super-infection with the cat virus, the transformed cells did not produce any virus particles.

Apparently, the cat leukaemia virus provides the transforming mouse sarcoma virus, resident in hamster cells, with missing factors necessary for the production of progeny sarcoma virus.

This experimental, animal-cell system obviously indicates how cat sarcoma and leukaemia viruses might be used to reveal a human tumour virus. Following the reasoning of McAllister *et al.*, suppose, for example, that human osteosarcoma cells are cancerous because they have been transformed by a human sarcoma virus, which is defective in the sense that it is unable to replicate and produce progeny virus particles. Such a cell would be equivalent to the hamster cell transformed by the mouse sarcoma virus. By superinfecting it with a mixture of cat leukaemia and sarcoma virus the defective human virus might be helped; in other words, the cat viruses might provide the missing functions necessary for the complete replication of the putative human sarcoma virus.

This is precisely the experiment that McAllister *et al.* have done; they superinfected with the two cat viruses a line of cells derived from a human osteosarcoma. After such transformation the growth properties of the cells are changed, they grow to greater densities, they require less serum and they also contain and yield virus particles. All the immunochemical and biological tests done so far on these progeny virus particles suggest they are identical to the feline leukaemia and sarcoma viruses used initially to super-infect the cells.

Research in the Serengeti

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New laboratories for the Serengeti Research Institute (Fig. 1) are about to be inaugurated by the President of Tanzania, Dr Julius Nyerere. This will be an important occasion for the study of ecology and for the future rational management of the Tanzania National Parks, and the article which follows surveys the scope of the institute's research programme.

THE Research Institute is located at the centre of the Serengeti National Park in the transition zone between the extensive woodlands dominated by *Acacia* and the great Serengeti Plain. The national park is approximately 13,000 km² and contains a rich and diverse fauna. More than fifty species of large mammals are found there in great abundance as well as an extremely diverse bird fauna. Many mammals are migratory (especially the numerous wildebeest, zebra and Thomson's gazelle) and they move far beyond the boundaries of the park, using an area of at least twice its size. The importance of the national park goes far beyond being a tourist attraction and an earner of foreign exchange for Tanzania. It is still a relatively natural ecosystem, at an early stage of degradation through human interference. This makes the Serengeti significant in the context of world wide concern about the despoilation of the environment, and lends urgency to the research carried out there for the purpose of conserving this unique area. The Serengeti is in a dynamic state, influenced by both climatic changes and increasing human interference inside and outside the national park. For proper conservation, active management interference will be imperative and the programme of the research institute is designed to provide background information for such management. Its objectives are to describe, monitor and analyse the changes taking place within the ecosystem, and to analyse the regulating mechanisms at work. Only on such information can the best policies for conservation and management be based.

The basic capital facilities of approximately £170,000 were donated mostly by the Fritz Thyssen Stiftung (Germany), the Old Dominion Foundation (USA), and the Kleberg Foundation (USA) and the recurrent costs of £75,000 per annum are provided largely by the Ford Foundation (USA) and the African Wildlife Leadership Foundation (USA). A further £60,000 recurrent expenditure is provided by overseas research grants to scientists working at the institute. The trustees of Tanzania National Parks have also supplied generous support.

The housing and research facilities provided at the institute for seventeen scientists include general laboratory equipment, library and herbarium, workshops and administrative offices. More specialized equipment includes aerial cameras, processing apparatus for black-and-white and colour film, stereoscopes and other mapping tools, a desk computer, calculators and card-punching equipment and radio-tracking material. There is also a fully equipped veterinary and

pathological laboratory, and two light aircraft (one equipped for aerial surveys), landrovers, lorries and other vehicles. Additional apparatus is brought by individual scientists.

The first ecological research project in the Serengeti ended in 1959 with the tragic death of Michael Grzimek. The present programme of ecological research was initiated in 1962 by the director of Tanzania National Parks, Dr J. S. Owen, who realized the urgent need for information on the interactions between the populations of large mammals and the habitat, and on the current trends within the ecosystem.

Study of Ungulates

The principal effort has been concentrated on the behaviour and ecology of the most common ungulates, especially on population dynamics and population regulating mechanisms, including the role of predation and disease. Scientists have been recruited for short term projects (lasting three to four years) which fit in with the overall research policy. Studies completed so far include the population of dynamics of wildebeest and buffalo, a preliminary study of interactions between herbivores and vegetation, the behaviour of zebra, gazelle and elephant, the ecology and behaviour of hyaenas and lions, veterinary and parasitological studies and a geological survey. About seventy publications concerned with plant and animal ecology, ethology and parasitology have been produced.

A few examples will illustrate the results obtained in studies already completed. R. M. Watson has studied the ecology, reproduction and population dynamics of wildebeest (*Connochaetes taurinus albojubatus* Thomas), the dominant herbivore in the Serengeti region, and described the distribution, movements and habitat utilization of this well known migratory species¹. As well as its value as a source of information on which the conservation planning of the Serengeti National Park could be based, this project shed further light on the concept of grazing succession in African ungulates first described by L. D. E. F. Vesey-Fitzgerald².

H. Klingel, in his study of the plains zebra (*Equus quagga boehmi* Matschie), gave one of the few detailed accounts to have been prepared of the social organization of a horse species³. His findings are of special interest in the context of ungulate biology, for they illustrate a social organization which, as an adaptation to a grazing life on the plains, is very different from that of the antelope species whose habitat zebras share so closely.

R. H. V. Bell has studied the utilization of different zones on the catena—a repeated sequence of soil types—by ungulate species to demonstrate their ecological separation, and also showed how the dominant grazing species select different parts of the grass plants or different growth stages⁴. A. R. E. Sinclair studied the ecology and behaviour of the African buffalo (*Syncerus caffer* Sparman) in the Serengeti⁵. This work contributed substantially to knowledge of the natural regulation of animal numbers through the density dependent action of direct food limitation, which affects primarily adult and juvenile mortality. Limitation of food apparently does not affect fertility in a density dependent manner.

H. Kruuk has described the role of predators in the regulation of ungulate populations⁶. He considered that spotted hyaenas (*Crocuta crocuta* Erxleben), although

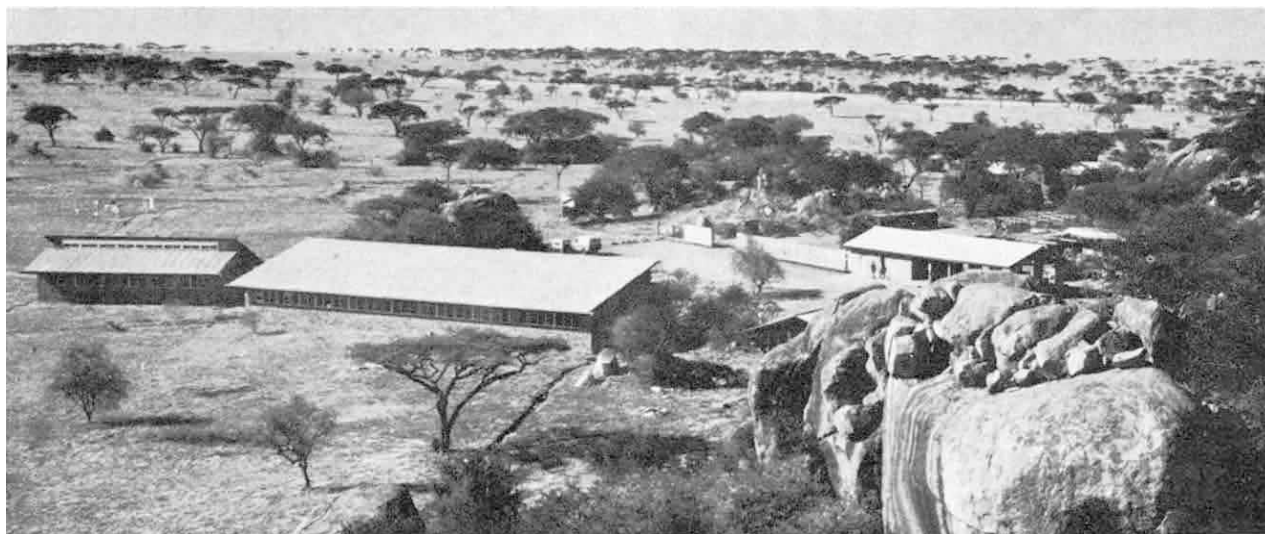


Fig. 1 Laboratories and other buildings of the Serengeti Research Institute.

important predators as well as scavengers, play a relatively minor part in the regulation of ungulate populations in the Serengeti: nearby in the Ngorongoro Crater, however, they are by far the most important cause of death and an effective limiting factor.

By studying the diseases and parasites of large mammals R. Sachs has contributed considerably to knowledge of the extent of pathological and non-pathological infections in the mammals of Serengeti. His work has been continued by A. Young and B. Schiemann. These and the ecological studies have shown that disease operates as a secondary mortality agent, speeding up the response of populations to the limited food supply.

Population Dynamics

Studies are in progress on the population dynamics and ecology of zebra, gazelle, impala and elephant; on the social behaviour of impala; on the grazing behaviour of wildebeest and other ungulates; on the effects of predation on the resident woodland game communities; on the role of vultures as scavengers and on the importance of diseases and parasites in large mammals. Botanical studies are in progress on the separate and combined effects of fire and elephants in the woodland vegetation of the park. The various vegetation types of the grasslands and woodlands are being described and mapped, and grassland productivity and woodland vegetation dynamics are being assessed in relation to edaphic and environmental factors. Basic to much of this work is a landscape classification which is being prepared with the help of aerial photography. Computer techniques are frequently applied, especially for setting up data banks, for the integration and analysis of data and for simulation studies.

These short term studies have provided many of the essential data on which management policies within the park can be based. They also provide the first insights into the complex interaction between the various ungulate populations and their habitat. The studies of the population dynamics of wildebeest, buffalo and possibly other ungulates suggest that the densities of these species are regulated through density-dependent mortalities and that reproductive rates remain independent of density. Predation studies show that predators play a very minor part in the regulation of numbers of these species. It is the interactions between the ungulates and their food supply which are of major importance. These interactions are complex: the different ungulate species not only compete for the same grass species but also seem to facilitate each other's grazing; one species may open up the vegetation for another. Even though the grassland

productivity is never fully utilized by the herbivores, there is mortality, probably caused by starvation, during the height of the dry season. Studies of population structure are showing which sections of the populations are most affected by this mortality.

Routine Monitoring

The study of interactions between ungulate populations and their habitat needs to be continued for many years, so that the effects of different stages of the climatic cycle and other environmental trends may be assessed. A long term monitoring programme has been designed to do this without the need for a great deal of scientific manpower. Changes in the climate, vegetation, numbers and structure of ungulate populations and their movements within the ecosystem are monitored by routine sampling. The techniques used are designed by specialists to ensure the collection of the most important information with minimum effort. Climate is monitored by means of forty rain gauges and other meteorological instruments distributed throughout the park, and by the use of data supplied by the East African Meteorological Department. Vegetation monitoring is based on a series of sample plots, growth plots, ground transects and aerial photographic transects. The sizes and structures of the populations of wildebeest, buffalo and elephant are assessed by aerial sampling throughout the year; the migratory movements of the ungulate populations in relation to range conditions (availability of grazing, incidence of fire, distribution of water) and human settlement are recorded during monthly survey flights which cover systematically about 30,000 km².

The objectives of this routine monitoring are to document and describe changes within the ecosystem to provide the essential continuity of data on which any long term study depends, and to make possible extrapolation of data collected by individual scientists to the entire ecosystem. It is with this last objective in view that the future research policy of the institute is formulated.

We are now in a position to make a concerted attack on what at present seems to be the most crucial problem for conservation and management, and for ecology from a more theoretical point of view—to discover the patterns of movements and habitat utilization within the ecosystem and the competitive interactions between the ungulate populations. The size of the area makes it impractical to study all parts of the ecosystem simultaneously and it is also felt that the methods used so far in studies of individual species would be uneconomical. We are therefore planning to concentrate research into three areas which are the most

important for both resident game and large migratory populations. A "regional team", with a nucleus of a zoologist and a botanist, will be assigned to each area. The botanist will study the productivity of the vegetation types, quantitatively and qualitatively, and its utilization, and make detailed vegetation maps. The zoologist will have as his principal objectives: to determine the utilization of different vegetation types by the various ungulate populations, to examine the competition and facilitation between ungulate species, and to relate these to the state of the vegetation, the phase of productivity, the extent of fire, the availability of water and human activity.

The regional teams will be backed by a "central team" consisting of the staff of the monitoring team, one research worker continuing observations on the population dynamics of the most important ungulates, a student of predation problems, a veterinarian, a soil scientist, a forest ecologist studying the dynamics of the various woodland communities, another studying the effects of fire, and one or two botanists specializing on quantitative vegetation description and mapping. Possibly there will also be a termite ecologist. In addition, there will be specialists on the interpretation of aerial photography and computer programming.

We are planning to pursue these lines of research for the next three to five years. The various elements of the programme will be closely integrated, with the data collected simultaneously by the regional teams, and in such a way that they are immediately comparable between regions. These coordinated data, when taken together with the over-

all picture gained from the monitoring programme, will allow extrapolation to the whole ecosystem. In this way we hope to make the preliminary analysis of the interactions and regulatory mechanisms of the community of plants and animals in the Serengeti. This analysis will also suggest where future research effort should be concentrated. Although we are encouraging qualified citizens of East Africa to take up both senior and junior research posts, we are still looking for research workers and finance for four of the botanical projects, and for one pedologist and one ecologist to study the utilization of vegetation by ungulates.

We believe that this programme, which is founded on the basis of the research already carried out and on the support for the Serengeti Research Institute from Tanzania and overseas, will realize the unique opportunities of ecological research offered by the Serengeti and will contribute substantially to the conservation of this wonderful area and others like it. It is imperative that the capital, experience and manpower so far invested in the institute shall be utilized to the full by ensuring continuity of research. The efforts of the director and staff will be directed towards these aims and towards making the institute an effective member of a net of ecological research stations.

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⁴ Bell, R. H. V., in *Brit. Ecol. Soc. Symp.*, No. 10 (1970).

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Standing with a Bent Knee

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When a man stands, the physiological control of his leg muscles is made simpler than might have been expected by the kinematic design of the knee. Experiments with a working model have shown that a muscle acting as a linear spring produces a propping thrust which is independent of the length of the leg.

ALTHOUGH man is a biped, he can, and often does, stand on one leg. If, at the same time, he inclines his trunk forward, he commonly holds his free leg out behind as a counterpoise, as in the pose of the celebrated statue of Mercury by Giovanni di Bologna. In this situation the two legs perform very different functions, which can be described as those of "prop" and "paddle" respectively. The same two functions can also be distinguished in the more commonplace acts of standing, walking, kicking a football or wiping the feet on a doormat. A prop resists a compressive load applied between its ends; a paddle exerts a turning moment because the line of action of the force against its blade is offset from the axis of rotation. The forces acting between the leg and the trunk can at all times be resolved into the corresponding two components—a compressive load acting through the hip joint together with a couple.

The magnitude and sign of the couple acting at the hip depend on various factors, such as how far away the centre of

gravity of the trunk and upper extremities lies from a vertical line through the hip joint, or whether an unsupported leg is being used as a counterpoise or is engaged in kicking or scraping. Other variations arise from inertia forces when the parts of the body are put into relative motion.

There are two sets of knee extensors: one acts only at the knee, the other also spans the hip joint. We can associate the single-joint muscles with propping and the two-joint muscles with paddle action, on the grounds that when the leg is shortened the two-joint muscles undergo compensatory changes at hip and knee (Fig. 1) so that they are not well placed to resist the compressive load. These muscles can, however, serve to transmit substantially the same turning moment from tibia to pelvis in spite of a change in the overall length of leg.

With the knee bent, the compressive load tends to bring the foot nearer to the hip. This tendency to shorten the leg is opposed by the muscles that extend the knee.

The separation of the muscle groups simplifies the control task to the extent that prop and paddle functions can be regulated independently. Considering the propping function in more detail, does the motor command signal to produce a particular resistance to compression of the leg have to be continually recomputed by the central nervous system and adjusted to take account of changes in the angle at the knee? I wish to show that the answer to this question is probably "no", indicating a further simplification of the control task.

If the knee is bent while the leg is supporting the weight of the body, several things alter simultaneously. Bending the knee increases the leverage with which the body's weight acting downward at the hip tends to flex the knee further. At the same time the position of the axis of rotation is shifted relative to the

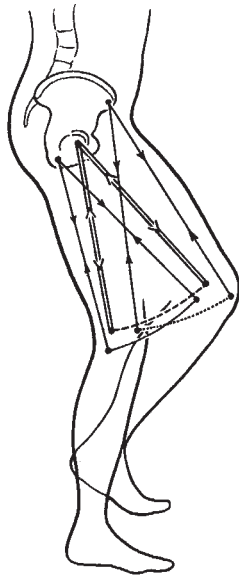


Fig. 1 Schematic diagram to indicate that when the foot is lifted neither the anterior nor the posterior two-joint muscles of the thigh need to alter in length.

bones, and the leverage for the line of action of the muscle force is altered. The muscle is stretched and consequently develops a greater tension both passively and reflexly. It is not, at first sight, easy to predict the balance between the effects of all these changes.

First it is necessary to define the position of the axis of rotation. The knee joint contains a number of ligaments which restrict the movement of the bones. Lateral bending is prevented by the medial and lateral ligaments on either side of the joint. When the knee is flexed, contraction of the hamstring muscles would tend to pull the tibia backward under the end of the femur were it not for the presence of the posterior cruciate ligament. The anterior ligament prevents the tibia from slipping forward when the pull of the quadriceps presses the patella downward on to the femur. If the quadriceps and hamstrings are voluntarily contracted in turn, with the foot on the floor and the knee bent, we do not usually observe the end of the tibia gliding backwards and forwards under the femur. From this we can conclude that the cruciate ligaments normally have very little slack in them. This is ensured at all angles of the joint by the special contour of the articular

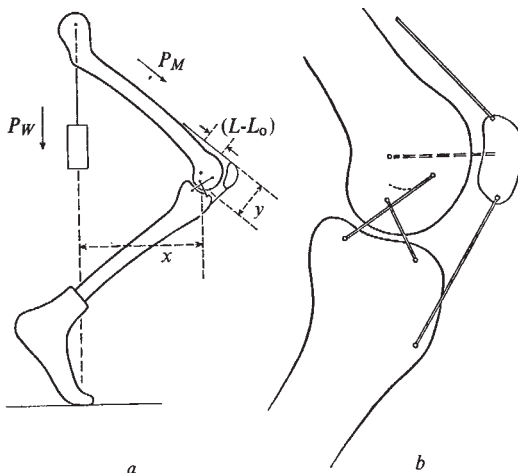


Fig. 2 a, Diagram of model leg to show how the offsets x and y were measured. b, Detail of the pivot points at the knee. The dotted line shows the bone profile round which the posterior ligament becomes wrapped in full flexion of the knee.

surfaces. Indeed, it can be shown that the precise contour of the articular surfaces is governed by the geometry of the lengths and points of attachment of the cruciate ligaments¹. It follows that the instantaneous axis of rotation for the relative movement of the bones of the knee joint must at all times pass through the lines of action of the two ligaments. If the lines of action of the ligaments are projected on a plane normal to the axis of rotation, the projections intersect in that axis. A sequence of such projections thus reveals the path or locus of the axis of rotation, relative to each of the bones.

The patella glides in a groove in the end of the femur but it is tethered to the tibia by the patellar ligament. The tension developed by the quadriceps muscle is thus transmitted to the tibia as it were over a pulley, but the axis of this "pulley" is not at any time coincident with the instantaneous axis of rotation at the joint.

In an attempt to evaluate the simultaneous actions of all these separate factors a working model of a human leg has been constructed. Profiles of the femur and tibia were obtained by projection and pivot-joints were fitted to correspond with the points of insertion of the cruciate ligaments. The ligaments themselves were represented by rigid struts, the compression of which simulated the action of the articular surfaces without incurring the complications of sliding friction. The model of the patella was supported on a rigid strut pivoted at a point corresponding to the centre of curvature of the groove in the end of the femur, thus again avoiding sliding friction. The lower end of the tibia was continued into a fixed profile of a foot with dorsiflexed toes (Fig. 2).

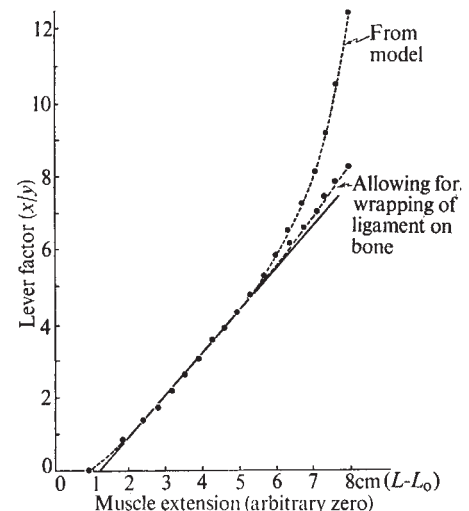


Fig. 3 Plot of x/y against $(L-L_0)$ for various angles at the knee joint in the model. Note that $P_M/P_W = x/y$, so that this graph gives the shape of the tension/extension diagram which a single-joint extensor of the knee would need to have if it were to be in equilibrium with a constant compressive load. For a linear spring $P_M = S(L-L_0)$.

The following measurements were then made from the model at various angles of the knee: the offset from the axis of rotation to the line of action of vastus crureus, the increment in the length of the muscle, the offset from the axis of rotation to the line of action of the compressive force (taken as running from the centre of the acetabulum to the point of contact of the ball of the foot with the ground). From these measurements an appropriate force/length relation for the muscle was derived. It turned out that, within a substantial range of angles, equilibrium with a given compressive load could be achieved by a simple linear spring (Fig. 3).

This surprising result was confirmed by the following practical test. The model was supported by a light framework of wires that prevented horizontal movement of the "hip" without constraining vertical movements. A steel coil spring was connected in place of the vastus crureus muscle and the

hip was loaded with an appropriate weight. It was found that this system remains in stable equilibrium in any of a wide range of positions. There is very little friction and only a very small force is needed to change from one position to another.

Many reflexly active muscles can be set to behave as approximately linear springs by a suitable pattern of impulses in the motor fibres to the intrafusal muscles of the muscle spindles—the fusimotor command²—and the important implication of the new finding about the kinematics of the knee joint is that, once the muscle has been set at a spring stiffness appropriate to the prevailing load, the fusimotor command to the muscle servo does not need to be adjusted every time the angle at the joint changes.

Where a change has to be made in the supporting force to be provided by a given limb the necessary adjustments can be achieved by simply altering the fusimotor command without the confusion that might arise if account had to be taken from moment to moment of the prevailing value of the angle at

each joint. This arrangement represents a substantial saving in the computational effort required from the brain. It will be interesting to see whether similar economies are effected at other joints and whether the reasoning developed here in relation to the single-joint muscles, used when the limb is acting as a prop against a compressive load, can be applied to the two-joint muscles when the limb is used in paddle action to transmit a turning moment to the trunk.

In engineering terms, the effect of the geometrical arrangements at the knee is to convert the force developed by a linear spring into a constant thrust exerted by a strut of variable length. The measurements on the model suggest that the success of the design depends more on the shape of the path of the patella over the femur than on the small movement of the axis of rotation that occurs under the constraint of the cruciate ligaments.

¹ Kapandji, I. A., *Med. Biol. Illus.*, **17**, 26 (1967).

² Matthews, P. B. C., *J. Physiol.*, **147**, 547 (1959).

Symposia in General

A correspondent offers some hints to organizers and participants preparing for the next round of scientific jamborees.

ATTENDANCE at symposia is nowadays a frequent and important event in the life of scientists. Yet the organization of symposia and the performance of the contributors often leave much to be desired. This is perhaps not surprising because there is no textbook or manual on how to plan, organize, contribute to, behave at and get the best out of symposia.

True there are several learned texts on how to speak as a scientist to scientists (for example, refs. 1–8). But something must be wrong with them because very little notice is taken of them. Besides, they cover only a limited aspect. So a new attempt to approach the subject from a broader point of view and from a fresh angle seems warranted.

First a few rules for the organizers. Above all provide value for money and cram into the available time at least twice as much as a normal human being can digest during a long working day. The absorptive capacity of scientists is almost unlimited and unless you give them a chance of exhausting themselves they might come away unhappy and feel that they have not done their duty.

Provide name labels for the eventuality that a participant has to identify someone, but print the names so small that they are illegible to the naked eye at 25 cm. Anonymity *vis à vis* unauthorized people must be safeguarded, and for reasons set out below participants are invariably equipped with efficient modern pocket binoculars and can therefore identify people if they so wish.

Do not tell participants in advance for how long they are expected to speak. This might be interpreted as intimidation and interference with the freedom of expression. Do not waste time on checking such trivialities as the functioning of microphones, the presence of a lectern, pointer, reading lights and means of communication between speaker and projectionist. Do not worry if the fan of the projector is so noisy that people in its vicinity cannot hear the speaker.

You may be familiar with Murphy's law: "If things go wrong they will go wrong". But do not let this worry you. There is no unanimity on what is wrong and what is right.

And now a few tips for speakers: cram into a slide as much as is photographically possible. Use the smallest lettering as participants have in their pockets the aforementioned binoculars, and remember that scientists are trained to assimilate any amount of slide material in a jiffy. You are in good

company if you stuff at least thirty lines into a slide. Do not forget that the purpose of showing a slide is to exhibit the wealth of data which you have amassed and to impress on the audience the profundity of your scholarship.

In an important publication entitled "Wie mache ich einen wissenschaftlichen Vortrag unerträglich?", Professor W. Leydhecker⁹ offers much valuable advice to speakers: At the beginning of your talk avoid at all costs attracting the interest of the audience. It is one of the well established aims to lull the audience into a deep and healthy sleep. One way to achieve this is not to begin at the beginning. You must start much earlier. First thank the organizers for the honour of being a participant and refer to the fine weather, beautiful scenery and good food. A well proven soporific method is a full historical list of all the authors in the field. Take literally the main purpose of your participation to "read" a paper and when writing it make your sentences beautifully long and elaborate. Never use a short word when a long one is available. Include plentiful quotations from the non-scientific literature in order to do justice to your profundity, sense of humour, wide culture and subtlety, and whenever possible quote in exotic languages.

Ignore the microphone as a confounded nuisance and address the blackboard—a very good listener—rather than your doubtful audience; speak in a low and monotonous voice and drop the voice at key points of the argument, as well as when proper names, unusual chemical substances or figures are mentioned. Above all do not allow yourself (when you have exceeded your allotted time) to be irritated by the bell of the chairman, by his hastily scribbled notes being thrust under your nose, or by other inappropriate interferences. Go on talking. You are the only person of importance in the room. The only concession, Leydhecker suggests, that you might make to the restlessness of the chairman is to talk a little faster. This will enable you to read the remaining ten pages of your talk in one or two minutes.

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⁴ Zollinger, R. M., and Howe, C. T., *Med. Bull. Illinois*, **14**, 154 (1964).

⁵ Hawkins, C. F., *Lancet*, **i**, 261 (1964); Clarke, P. R. R., *ibid.*, **334**; Cameron, J. K., *ibid.*, **385**; Treadgold, S., *ibid.*, **385**; Astley, D. J. B., *ibid.*, **493**; Witts, L. J., *ibid.*, **493**.

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⁷ Bragg, L., *Science*, **154**, 1613 (1966).

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Adsorption on the Martian Regolith

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Laboratory measurements of the adsorption of CO₂, H₂O and Kr indicate that pulverized basalt has an extremely high internal surface area. This suggests that the Martian regolith may store an appreciable amount of adsorbed gas, and that the diurnal brightening of Mars may be caused by a fog of ice crystals formed from water molecules desorbed diurnally from the surface.

STUDIES of particle sizes in lunar soil¹ show that pulverization by meteorites has produced a regolith with a high geometric surface area. The meteorite flux on Mars may have been up to twenty-five times that on the Moon². Thus the ablation products of meteorites and aeolian comminution near the surface are likely to have produced a fine grained regolith on Mars. Naturally, regolith adsorption will be more important in a tenuous atmosphere such as that of Mars than in a more substantial atmosphere. Davis³⁻⁵ has suggested that a substantial amount of CO₂ might be adsorbed in the bright areas of Mars and might be of importance for the CO₂ metabolism of possible martian organisms. The purpose of our study was to measure CO₂ and H₂O adsorption in conditions and on materials likely to be representative of the martian surface. Based on these data, we considered the significance of adsorption for the history of H₂O and CO₂ on Mars, and for the diurnal phenomena.

We measured the CO₂ adsorption at -77° , 0° and 29° C on Vacaville basalt pulverized in a vacuum. (The reflectance spectrum of Mars is compatible with a basaltic surface composition⁶, and the temperature range studied corresponds to most of the diurnal and seasonal range of martian surface temperatures⁷.) Measurements were made at CO₂ partial pressures from 1 to 20 torr, covering the range of surface CO₂ pressure considered possible⁸. Basalt samples, weighing approximately 6 g, were vacuum-pulverized for 0.5 h in an evacuated 347 stainless-steel ball mill which was then attached to an apparatus for measuring the CO₂ adsorption and effective surface area⁹. The amount of CO₂ adsorption in the ball mill when empty was negligible at each temperature. The CO₂ isotherms obtained are shown in Fig. 1. Water adsorption was measured by equilibrating the pulverized basalt with various partial pressures of H₂O at 29° C and monitoring the weight gain as a function of water pressure and time (Table 1). The data indicate that the adsorptive capacity of pulverized basalt is very high; at 5 mm CO₂ and -77° C, a gram of pulverized basalt can adsorb almost 1 cm³ (at STP) of CO₂ and, at 29° C, 8.5 mg of H₂O.

This is because the "true", or effective, surface area of silicate grains can be much larger than their strictly "geometrical", or ostensible surface area because of surface involution or porosity¹⁰. The geometrical area can be estimated from the particle size distribution. Assuming the pulverized basalt sample to consist of randomly packed spheres of density 3.0 g cm⁻³ then the geometric surface area per gram,

S_g , is given by $S_g = 8/3d$ where d is the particle diameter¹¹. Suppose we let d be the mean of all particle diameters, weighted according to mass. The weighted mean diameter of the basalt after grinding was 0.022 mm, and the corresponding geometrical surface area was 0.12 m² g⁻¹. The true surface area of the sample was determined by Kr adsorption at -195° C (ref. 9) by a procedure based on an extension of the Langmuir theory of adsorption¹² that is due to Brunauer *et al.*⁴. According to their multilayer adsorption theory, the equation for a free surface is

$$\frac{P}{V(P_0 - P)} = \frac{1}{V_m C} + \frac{(C-1)}{V_m C} \cdot \frac{P}{P_0}$$

where V is the volume of gas adsorbed at the equilibrium partial pressure P . For a given adsorbent-adsorbate system at a given temperature, V_m and C are constants related to the true surface area and the heat of adsorption, respectively, and P_0 is the saturation pressure at that temperature. Thus, theoretically, a plot of $PV^{-1}(P_0 - P)^{-1}$ versus P/P_0 (a BET plot) after the authors of ref. 4 yields a straight line with intercept $(V_m C)^{-1}$ and slope $(C-1)(V_m C)^{-1}$. The quantity V_m is the volume of gas adsorbed per monolayer per gram. From V_m , the true surface area, S_T , is given by

$$S_T = \frac{10^{-20} V_m N \text{ \AA}^2}{n} \text{ m}^2 \text{ g}^{-1}$$

where $\text{\AA}^2$ is the area (in square Angstroms) of the adsorbate molecule, n the volume of adsorbate per mole in cm³ at STP, and N is Avogadro's number. We obtained a linear BET plot for the pulverized basalt (Fig. 2) corresponding to a true surface area of 5.8 m² g⁻¹, fifty times higher than the geometrical area. If the sample consisted of uniform spheres which were not involuted or porous, they would have to have a diameter of 0.4 μ m for the sample to have the observed surface area. The scanning electron microscope revealed some surface subdivision down to a 0.1 μ m scale and below, but whether the subdivision is continuous and interconnecting

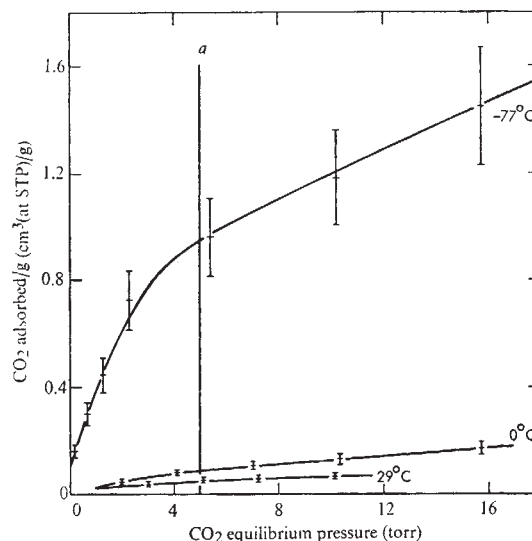


Fig. 1 Adsorption of CO₂ on vacuum-pulverized Vacaville basalt at 29, 0, and -77° C. *a*, Average partial pressure of CO₂ at base of martian atmosphere.

Table 1 Water Adsorption on Pulverized Vacaville Basalt at 29° C

Relative H ₂ O pressure (P/P_0)*	Milligram of H ₂ O adsorbed per g	Micromols of H ₂ O per m ² ($S_T = 5.8 \text{ m}^2/\text{g}$)	No. of monolayers of H ₂ O adsorbed
0.13	0.5 ± 0.2	5	3
0.28	2.3 ± 0.2	22	14
0.52	4.5 ± 0.2	43	27
0.99	8.5 ± 0.2	81	52

* Relative H₂O pressure is defined as the ratio of the ambient H₂O pressure at any temperature to the saturation vapour at that temperature.

throughout the grains cannot be established from the photographs.

Using the relationship given here and assuming a true surface area of $5.8 \text{ m}^2 \text{ g}^{-1}$ then at relative pressures of 0.13, 0.28, 0.52 and 0.99, there are respectively approximately 3, 14, 27 and 52 monolayers of H₂O adsorbed in equilibrium on the pulverized basalt surface (Table 1). Usually, the relative water pressure determines the number of H₂O monolayers adsorbed and, together with the internal surface area, the mass of water adsorbed. Moderate variations of temperature have little effect on the amount of adsorption as long as the relative pressure of water is constant—especially at high relative pressure¹³. Therefore, the amount of water adsorbed on pulverized basalt in equilibrium with various H₂O pressures can be estimated roughly for various temperatures based on the room temperature data in Table 1.

The average blackbody surface temperature of Mars, integrated over the disk, is approximately 210 K according to the 3.15 cm microwave emission, which is in agreement with theoretical estimates of the martian subsurface temperatures assuming Mars to be an ideal radiator¹⁴. We assume a regolith depth of 10 m and a bulk density of 1.5 g cm^{-3} for the martian regolith. At 210 K, there would be approximately 6 cm^3 (at STP) of CO₂ per gram adsorbed in equilibrium (Fig. 1), or approximately $1,000 \text{ cm}^3$ ($\sim 2.0 \text{ g}$) adsorbed per cm^2 of the regolith column.

To estimate the amount of water adsorption, it is merely necessary to assume that a high relative humidity ($P/P_0 > 0.5$) prevails within much of the surface regolith. Schorn *et al.*¹⁵ have observed average water contents in the northern hemisphere of up to 25 μm of precipitable water. Together with evidence on the vertical distribution of water in the martian atmosphere, this suggests that the lower martian atmosphere would be near saturation at temperatures as high as 225 K (ref. 17); in other words at temperatures above nightly lows on Mars⁷, even near the equator. At high relative humidity, as much as 52 monolayers or approximately 9 mg g^{-1} of adsorbed water would be in equilibrium with the basal martian atmosphere, as much adsorption as expected for a tropical rain forest on Earth.

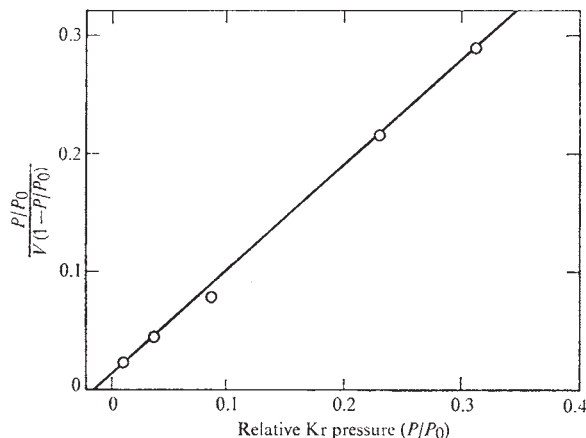


Fig. 2 BET plot for vacuum-pulverized Vacaville basalt (Kr adsorption at -195°C).

Mars				Earth
H ₂ O	CO ₂	H ₂ O/CO ₂	H ₂ O/CO ₂ Total atmosphere and regolith	H ₂ O/CO ₂ in Rubey surface volatile inventory
Martian atmosphere (observed)			1.2 (see text)	18
$\sim 2 \times 10^{-3} \text{ g/cm}^2$	10 g/cm^2	2×10^{-4}		
Martian regolith (estimated, this work)				
$\sim 15 \text{ g/cm}^2$	2.0 g/cm^2	7.5		

Fig. 3 Estimated partition of H₂O and CO₂ between the martian atmosphere and the adsorbed volatile inventory in the martian regolith. The estimated total (atmosphere+regolith) volatile inventory of Mars is compared with the total surface inventory on Earth (Rubey inventory).

These values have important implications for the volatile budget of the martian surface (Fig. 3). According to Rubey¹⁶, the H₂O/CO₂ mass ratio in the volatiles released to the Earth's surface during geological time is approximately 18. In contrast, the H₂O/CO₂ ratio observed at present in the martian atmosphere is $\sim 2 \times 10^{-4}$. There are of course no large bodies of liquid water on Mars, and the (very thin) polar caps are chiefly CO₂ (ref. 17). The site of the "missing" water (missing relative to the atmospheric CO₂ on Mars) has been much discussed. Based on our estimates, a martian atmospheric H₂O/CO₂ ratio of 2×10^{-4} would correspond to or, more exactly, be in equilibrium with, an adsorbed H₂O/CO₂ ratio of 7.5 in the regolith—similar to the Rubey value for Earth. Although we do not deny the probable importance of other fractionating processes, such as simple freezing, our results suggest that much of the discrepancy between the H₂O/CO₂ ratios for Mars and Earth must be attributed to martian regolith adsorption.

Our work here may also have a bearing on the surface brightening phenomena which have been observed repeatedly on Mars but never satisfactorily explained. Leighton *et al.*¹⁷ point out that fluctuations in the extent of the brightening of the northern polar cap region suggest cloud or haze, and the fixed locations of bright areas indicate either frost or orographically fixed clouds. An argument against frost, which they point out, is that the greatest brightness develops in the martian forenoon, when the surface is hotter than the atmosphere above. They also say that condensation and evaporation would be so slow that, if the brightening was caused by clouds, then they should be streaked and distorted by martian winds. This effect was not observed by Mariners 6 and 7, however. Finally, they point out that localized permafrost cannot be the source of the clouds because the permafrost would be exhausted in a few hundred years. They conclude that the brightenings seen in the tropics and subtropics cannot be explained easily by mechanisms involving H₂O. But Leighton *et al.* did not discuss other mechanisms such as replenishment from subsurface liquid water or a reservoir of adsorbed water. We suggest that the diurnal brightening may be attributed to the formation of ice fog as the direct result of desorption of water from the regolith.

In equilibrium, if the relative H₂O pressure drops from $P/P_0 \sim 1.0$ to $P/P_0 < 0.25$, regolith dust will lose about 6 mg g^{-1} of adsorbed water (Table 1). If the pore gas and the local atmosphere were saturated at a night-time low of -78°C , the water would have a pressure of $5.6 \times 10^{-4} \text{ torr}$. If the only way that water enters the system is from the soil, the diurnal rise in temperature will produce a drop in the P/P_0 of the pore gas by greatly increasing P_0 at nearly constant PH_2O . An increase of only 10°C (from -78° to -68°C) will raise the equilibrium saturation vapour pressure to $2.6 \times 10^{-3} \text{ mm H}_2\text{O}$, thereby dropping P/P_0 in the hypothetical closed system

by the required factor of four and desorbing most of the water. Thus, the depth to which the increase in temperature from the night-time low to the forenoon high will desorb most of the soil water is that at which $\Delta T \sim 10^\circ \text{C}$. In the equatorial region the temperature imposed at the top of the regolith rises from a night-time low of -60° to -80°C to an afternoon high of approximately 0° to 20°C . The depth, X , at which the diurnal temperature variation at the surface produces a responsive variation of $\Delta T/e$ is given by $X \simeq (\omega \rho C / 2K)^{-1/2}$, where ρ is the bulk regolith density ($\sim 1.5 \text{ g cm}^{-3}$), ω the frequency of the diurnal cycle (the diurnal period $\infty \sim 9 \times 10^4 \text{ s}$ for Mars), C the specific heat ($\sim 0.2 \text{ calories g}^{-1} \text{ degree}^{-1}$ for typical rock-forming minerals), and K is a constant related to the grain size, pore gas pressure and composition. For a pore gas pressure of 5 mbar CO_2 and a grain size of 0.1 mm, $K = 9 \times 10^{-5} \text{ calories cm}^{-1} \text{ s}^{-1} \text{ degree}^{-1}$ (ref. 18). Using these values and taking $\Delta T = 90^\circ \text{C}$ at the surface, most of the adsorbed water in the top 14 cm of the regolith will be desorbed. Assuming a regolith bulk density of 1.5 g cm^{-3} , a column depth of 14 cm, and a desorbable water content of 6 mg g^{-1} ; 120 mg of water will be desorbed into the lower atmosphere by the diurnal per cm^2 of regolith area. This amount of water, if present as ice fog (equivalent to a path length of ice of 1.2 mm), would certainly be visible. We feel that our source of the clouds largely overcomes the objections of Leighton *et al.*¹⁷ which are that local permafrost sources would be exhausted in a few hundred years and would sublime so slowly that clouds would be "stretched" by winds. We find that more than two-thirds of the water adsorbed on the pulverized basalt could be desorbed within 90 min at 29°C after desiccation, which would allow the development of ice fog in relatively still, orographically protected areas where the brightening is most frequently observed. To the extent that martian winds

do occur in these areas, they would be likely to provide a mechanism for H_2O replenishment in the topmost regolith each evening, and greatly increase the effective depth to which the diurnal desorption occurs (over our estimate of 14 cm).

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Mitochondrion as a Multifunctional Organelle

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Mitochondria are the site of much more than electron transport and oxidative phosphorylation. This should not be forgotten if mitochondrial function is to be understood.

THAT the mitochondrion is the site of the electron transport chain and oxidative phosphorylation is well known and, indeed, the mechanism of oxidative phosphorylation is an active area of biochemical research. Within the outer mitochondrial membrane, however, many other kinds of metabolic reactions also occur. Thus, mitochondria contain enzymes involved in the Krebs cycle, the β oxidation of fatty acids, fatty acid biosynthesis¹, synthesis of some amino-acids², lipid catabolism, the urea cycle and glutamate and aspartate biosynthesis³ as well as several oxidative enzymes not directly involved with oxidative phosphorylation, such as rotenone-insensitive NADH, cytochrome c reductase, monoamine oxidase and kynurenine hydroxylase⁴. This list is not exhaustive and will no doubt be extended in the years ahead.

Many of these enzymes or sequences of enzymes are biosynthetic or catabolic and consequently have end products not

directly involved in energy metabolism, so that the mitochondrion has a biosynthetic and catabolic as well as an energetic role, which would suggest that unless all mitochondrial end products—biosynthetic, catabolic and energetic—can be provided by alternative metabolic systems, mitochondria are essential for cell survival in all conditions. This conclusion is rarely discussed in general articles on mitochondria and infrequently recognized in research reports on mitochondria. Thus a recently published article⁴ began: "Mitochondria are DNA-containing, self-replicating organelles the functions of which are dispensable to yeast cells grown in the presence of a fermentable energy source". The issue is especially significant in the question whether or not anaerobic yeast cells possess mitochondria.

There are conflicting reports of the presence of mitochondria in anaerobically grown yeast cells. Electron microscopical studies suggest that anaerobic yeast cells lack mitochondria^{5,6}, but several workers have reported that if lipid precursors are included in the defined growth medium, mitochondrial profiles can be seen in electron micrographs of anaerobically grown yeast cells, although some disagree⁷.

Linnane *et al.* did not observe mitochondria in electron micrographs of anaerobic yeast cells⁸, but they did recognize "reticular membranes" which appeared to differentiate into mitochondria in aerobic conditions (see also refs. 5, 7), and detailed biochemical studies^{9,10} have confirmed their homology

with mitochondria. Although the reticular membranes lack the enzymes and cytochromes for oxidative phosphorylation, they do possess mitochondrial DNA, mitochondrial ATPase and succinate dehydrogenase. These mitochondria have a similar—if not identical—"structural protein" fraction to those isolated from aerobic cells. These membrane structures in extracts from anaerobic cells possess similar percentages of extracted protein to those in extracts from aerobic cells⁹, giving unequivocal biochemical evidence that mitochondria exist in anaerobic yeast cells, as would be expected from knowledge of their diverse functions.

In spite of this demonstration of the similarities between the membrane structures found in anaerobic yeast cells and the mitochondria in aerobic yeast cells, the membrane structures were called "promitochondria", not mitochondria, because they had neither the appearance of aerobic mitochondria in the electron microscope nor the ability to catalyse oxidative phosphorylation, but gained both these properties when the cells were exposed to oxygen^{9,10}. I suggest that because the area of specialized cytoplasm enclosed by the outer mitochondrial membrane has diverse metabolic functions, the mitochondrion is incompletely defined by its appearance in the electron microscope and its oxidative phosphorylation activity. "Promitochondrion" is, therefore, inappropriate. It seems more appropriate to consider the units of specialized cytoplasm, organized within outer mitochondrial membranes around the mitochondrial DNA of anaerobic cells, as mitochondria lacking some of the components found in mitochondria of aerobic cells. Equally extreme variation in mitochondrial composition is found in some muscle cells, in which the synthesis of inner membrane and oxidative phosphorylation proteins is highly derepressed in response to large demands for ATP¹¹ and considerable variation in mitochondrial enzyme composition is expected from knowledge of the diversity of mitochondrial metabolism. Enzyme levels are frequently regulated by the concentration of their specific end product(s), so that in cells with different requirements for particular mitochondrial end products, mitochondria of differing enzyme composition are expected¹² and tissue specific differences in mitochondria from the same organism have been described¹³.

Changes that occur in mitochondria after the transfer of yeast cells from an anaerobic to an aerobic environment, or from high glucose repression to low glucose repression, have been studied^{14,15}. In these conditions a number of mitochondrial components, particularly those concerned with energy production, are preferentially synthesized in a process described as "mitochondrial biogenesis". This term has also been used in studies of the synthesis of mitochondrial components in animal cells¹⁶⁻¹⁸. It does not imply the synthesis of new mitochondria from pre-existing mitochondria as is almost certainly the case in all eukaryotes. Each mitochondrial component may be formed *de novo*, but formation of new organelles is by growth and division of pre-existing mitochondria. I suggest, therefore, that "mitochondrial differentiation" is more appropriate to describe the changes that occur when yeast cells are transferred from an anaerobic to an aerobic environment.

Descriptions^{20,21} have been published of mitochondria isolated from "petite" strains of yeast which lack mitochondrial DNA, or the mitochondrial DNA of which has a severely restricted information content because it is composed almost entirely of adenine and thymine. Such mitochondria, which are of a similar size and buoyant density to those in the "grande" parent strain, lack the components of the electron transport chain but possess adenosine triphosphatase and other mitochondrial enzymes as well as an inner membrane²¹. Perlman and Mahler²¹, therefore, raised the question of why mitochondria continue to be synthesized when their oxidative phosphorylation capacity is lost. Knowledge of the other essential functions of mitochondria provides the answer.

The presence of mitochondria in cells lacking mitochondrial

DNA implies that in yeast the role of mitochondrial DNA in maintenance of mitochondria is limited, that is, the organelle is not genetically autonomous.

It has been suggested (refs. 22 and 23 and article in preparation by Flavell and Raven) that mitochondria have evolved from a prokaryote, able to catalyse oxidative phosphorylation, which sheltered in a primitive amoeboid-like cell and became stabilized there in a symbiotic relationship. At some time before the presumed symbiosis was established, the host cell must have been capable of living without the prokaryote, which would have been genetically autonomous. Since then considerable co-evolution of host cell and prokaryote has taken place.

The prokaryote has lost almost all its genetic information. Organization and compartmentalization have evolved in the eukaryotic cell such that the mitochondrion is now a fully integrated unit of the cytoplasm. Many biosynthetic and catabolic activities—as well as oxidative phosphorylation—must occur inside the outer mitochondrial membrane to be useful to the cell. This is well illustrated by auxotrophic strains of *Neurospora* which have metabolic deficiencies at the malate dehydrogenase step of the Krebs cycle, and other auxotrophic strains which have a metabolic block at the dehydroxy acid dehydratase step in isoleucine and valine biosynthesis. These strains, however, possess considerable malate dehydrogenase and dehydroxy acid dehydratase activity respectively, but possess metabolic blocks, because these enzymes are not localized in the mitochondria as in prototrophic strains, but remain outside the organelle in the cytoplasm^{24,25}.

Although multienzyme complexes can be isolated intact with the mitochondria, this physical association of related enzymes is probably the result of evolution rather than remains of a former autonomy. The present-day mitochondrion cannot be viewed as a symbiotic prokaryote or in any sense as self-sufficient, but is similar to other cell compartments or organelles such as Golgi bodies, glyoxysomes, microbodies and so on, which, although not having a prokaryotic origin, have evolved, as has the mitochondrion, to facilitate cytoplasmic organization, substrate and product channelling and metabolic pathway isolation.

Recently, great research emphasis has been placed on the electron transport system, oxidative phosphorylation, mitochondrial DNA and mitochondrial protein synthesis, which together with the endosymbiotic theory for the origin of mitochondria has, I think, propagated a view which fails to recognize the mitochondrion as a specialized area of cytoplasm of diverse functions and variable composition, the presence of which does not depend on mitochondrial DNA and is essential for eukaryotic life, even in anaerobic conditions. These features, however, cannot be ignored if we are to obtain a proper understanding of mitochondrial structure, function and replication.

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Some Aspects of the Evolution of Hearing in Vertebrates

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Comparative anatomical and electrophysiological studies can trace the evolution of vertebrate hearing.

DISCUSSIONS of the evolution of vertebrate hearing systems have tended, for lack of physiological data, to concentrate on changes in middle ear structure¹. Although the vestibular part of the inner ear of vertebrates has changed very little in evolution, it is possible to deduce from modern vertebrates that great changes have taken place in the auditory part. Conspicuous differences are seen among reptiles, birds and mammals in the shape of the basilar membrane (Fig. 1). Lack of data on non-mammalian vertebrates has limited comparative generalizations about the significance of these differences.

Generally, the basilar membrane is shortest in reptiles, longer in birds and longest in mammals, with some overlap. Elongation of the membrane presumably occurred (perhaps a number of times) in the evolution of the avian and mammalian stocks. The "cochlea" in birds and reptiles is only slightly curved, in contrast to the tightly coiled cochlea of therian mammals.

Several lines of evidence indicate that frequency analysis in the mammalian and avian inner ear takes place in such a way that low frequencies affect chiefly the apex and high frequencies the base of the basilar membrane. The orderly projection of auditory fibres to the brain^{2,3} maintains this separation as a tonotopic organization of auditory neurones in the brain.

It is now possible to bring together comparative data for mammals⁵, birds⁴, and reptiles^{6,7}. Although attention has been devoted to fishes and amphibians, these groups will not be discussed. Two important parameters of hearing, frequency response and sensitivity, will be used in this comparison.

Correlates of Audibility Curves

An audibility curve for the canary^{4,8} derived by conditioning techniques is shown in Fig. 2. Such behaviourally derived audibility curves have been produced for a number of mammalian species⁵, but for few species in other groups.

In all vertebrates for which electrical activity in the cochlear nucleus has been studied, it has been found that almost all auditory neurones are most sensitive to one frequency, the characteristic frequency. A frequency/intensity response curve for each neurone is V-shaped. When such "tuning" curves are obtained for a large sample of neurones, the curve produced by connecting their thresholds through the whole range of characteristic frequencies matches the behavioural audibility

curve extremely well (Fig. 2). This is also shown by data from the starling^{4,9} and the cat¹⁰.

Where behavioural data are lacking, single-neurone "audibility curves" can, for some purposes, be an effective substitute. The micro-electrode techniques for obtaining the single-neurone data from birds and reptiles have been fully described elsewhere^{4,6,7}. Calibration of sound intensity makes it possible to obtain the threshold sensitivity in absolute terms. Until recently, most data on hearing in reptiles (and birds, to some extent) were derived from cochlear microphonic recordings¹¹⁻¹⁶. So far, it is not clear what this response represents, and attempts to derive audibility curves from it have been fruitless¹⁷.

Table 1 gives the species of birds and reptiles used in this analysis. Data on all species of birds (mainly song birds) were made available by M. Konishi; other authors as noted. Reptilian data are my own^{6,7}, and from other authors as noted. These reptiles were chosen partly because the inner ear structures covered the range of the known morphological variation^{18,19} (Fig. 1).

Thresholds of Response

Single-neurone audibility curves are shown in Fig. 3 for six species of reptiles. As in birds and mammals, the hearing system is most sensitive at a certain frequency, the best frequency. This best frequency should not be confused with the single-neurone characteristic frequency. The intensity coordinate of best frequency is the lowest threshold.

Reptiles are most sensitive to quite low frequencies. The highest single-neurone characteristic frequency found in these

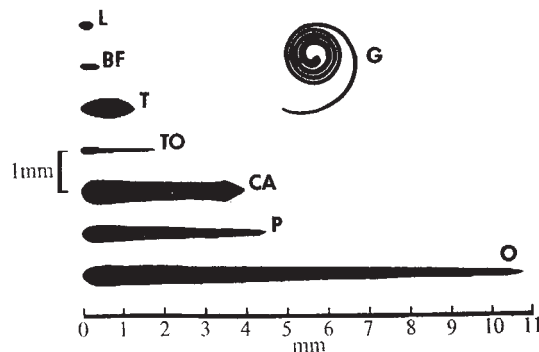


Fig. 1 Diagrammatic representation of the dimensions of the basilar membrane in some reptiles, birds and mammals. For abbreviations see Fig. 6. All membranes are drawn to the same length and width scales and slight curves have been represented as straight in bird cochleae. The guinea-pig membrane is 18.5 mm long.

Table 1 Avian and Reptilian Species included in the Analysis**Birds**

Order Passeriformes (song birds, various families)

House sparrow (*Passer domesticus*)Chipping sparrow (*Spizella passerina*)White-crown sparrow (*Zonotrichia leucophrys*)Slate-coloured junco (*Junco hyemalis*)American robin (*Turdus migratorius*)Starling (*Sturnus vulgaris*)⁹Canary (*Serinus canarius*)⁸Western meadowlark (*Sturnella neglecta*)

Family Columbidae (pigeon-like birds)

Pigeon (*Columbia livia*)^{34,35}

Family Tytonidae (barn owls)

Barn owl (*Tyto alba*)**Reptiles**

Order Chelonia (turtles and tortoises)

Gulf-coast box turtle (*Terrapene carolina major*)Pond-slider turtle (*Pseudemys scripta*)²²

Order Crocodilia (alligators, caiman, crocodile, gavials)

South American alligator or spectacled caiman (*Caiman crocodilus*)

Order Squamata (lizards and snakes)

Sub-order Lacertilia (lizards)

Family Iguanidae (spiny lizards and so on)

Leopard lizard (*Gambelia wislizenii*)Blue fence lizard (*Sceloporus cyanogenys*)

Family Gekkonidae (geckos)

Western banded gecko (*Coleonyx variegatus*)²³Tokay gecko (*Gekko gekko*)

six species was in the tokay gecko—5 kHz. Furthermore, not all species are equally sensitive at their best frequency, the geckos being much more sensitive than the box turtle or leopard lizard.

Fig. 4 gives comparative data for lowest threshold in mammals, birds and reptiles. The mammalian and avian systems are about equally sensitive, as noted by Masterton *et al.*⁵; but the reptiles are on average less sensitive. This may be due partly to lower body temperatures, but the measurements were made in each case at or near the thermal preferendum for each species, if known²⁰. Those reptiles with more differentiated basilar membranes are the most sensitive, suggesting a correlation between sensitivity and the number of hair cells, in spite of cochlear microphonic evidence to the contrary²¹. The middle ear transformation ratio (unknown in reptiles) may also be a factor in the poor sensitivity. In

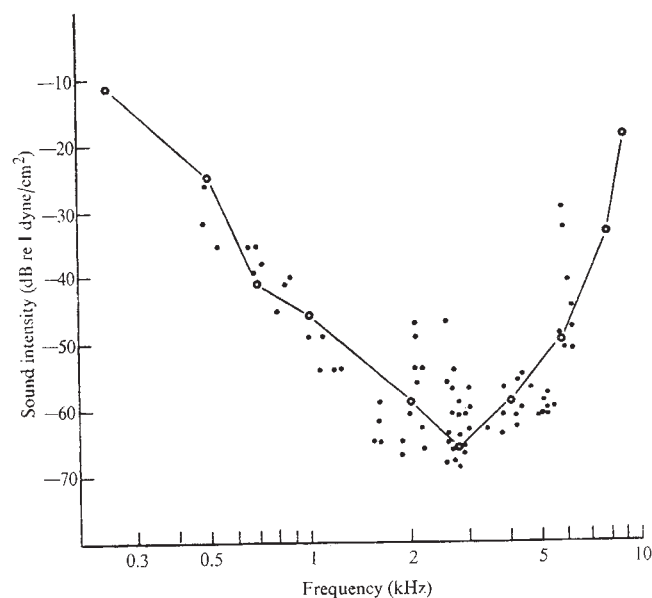


Fig. 2 Diagram (adapted from ref. 4) comparing audibility curves derived from behavioural (continuous line) and single-neurone techniques in the canary. Each dot represents the threshold of one neurone at its characteristic frequency. All neurones are from one individual.

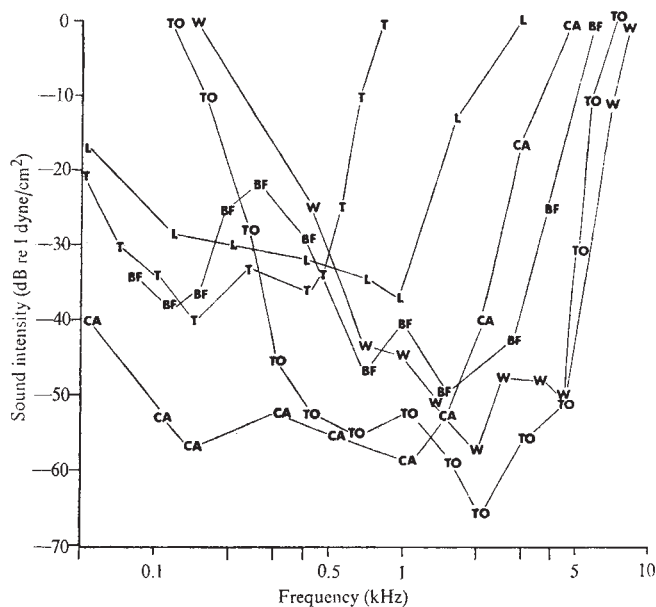


Fig. 3 Audibility curves for six species of reptiles derived from single-neurone neurophysiology^{6,7}. For abbreviations see Fig. 6.

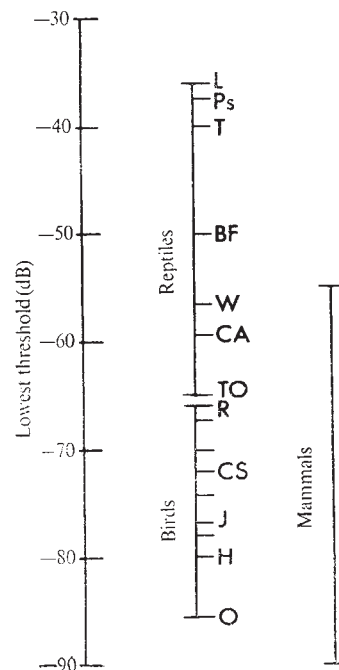


Fig. 4 Distribution of lowest threshold among reptiles, birds and mammals. dB scale is with reference to 1 dyne/cm². H, House sparrow; J, junco; CS, chipping sparrow; Ps, box slider turtle; for other abbreviations see Fig. 6. Mammalian range is from ref. 5.

mammals, the best frequencies range from 1 to 40 kHz (Fig. 5). Birds, however, form a compact group with a range from 1.8 to 4 kHz. Some of the larger birds when examined may, however, prove to have best frequencies outside this range. In reptiles the range is from 140 or 400 Hz (about equally sensitive in *Terrapene*) to 2 kHz in the geckos^{6,7,21,22}.

High Frequency Sensitivity

Conspicuous differences appear in a comparison of high-frequency cut-off in the three groups. The frequency where the audibility curve intersects the 0dB (with reference to 1 dyne cm⁻²) level on the high frequency side is chosen as the reference point. Among those mammals studied, only man and the apes show a cut-off frequency lower than 32 kHz,

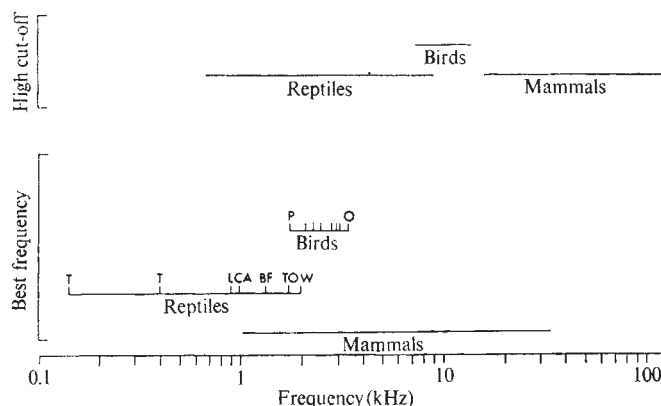


Fig. 5 Distribution of best frequency and high frequency sensitivity among reptiles, birds and mammals. For abbreviations see Fig. 6. Either value of T could be correct (140 or 400 Hz). Mammalian range from ref. 5.

but there are few data for the larger mammals. Several authors have speculated as to whether this sensitivity to high frequencies in mammals depends on the unique ossicular chain of the middle ear⁵. Mössbauer studies on the ability of different types of middle ear (mammal, bird, reptile and amphibian) to transmit high frequencies do show that the mammalian system is more efficient above a few kHz (K. Taylor, personal communication). In a few birds, high frequency cut-offs are as high as 12–13 kHz. In reptiles, all are below about 9 kHz. Data on high frequency cut-off are shown in Fig. 5.

In reptiles it has been shown that there is a correlation between the highest characteristic frequency found in the cochlear nucleus of a species and the dimensions of the basilar membrane⁷. In that case the dimensions of the basilar membrane were represented as length/width², that is, a ratio of the shape of the membrane (L/W) to its size (represented by the width, W). For membranes of the same shape, the wider membrane has a lower range of characteristic frequencies.

The widest part of the membrane was used in the above calculations, but when comparing reptilian, avian and mammalian membranes it is necessary to take into account the gradient of width. This gradient can be represented if the broadest part (W_w) and the narrowest part (W_n) are included in the formulation. Accordingly, in the analysis below each membrane is represented by the ratio of its shape to both widths (L/W_w : W_w/W_n). This is still an incomplete and somewhat artificial representation. But highest membrane values represent long membranes which are relatively narrow at both ends.

In the case of man, the highest characteristic frequency was estimated from behavioural audibility curves, but the high end of the curve is so steep that little error ensues. The highest characteristic frequency for other species is derived from single-neurone data^{4,7,10,23–25,34} (and D. Eldredge, personal communication). The other requirement for this analysis was an accurate knowledge of the basilar membrane dimensions^{6,7,18,26–30}.

The basilar membrane values and highest characteristic frequency for each species are plotted logarithmically on Fig. 6. The general conclusion derived for reptiles⁷ seems to apply to all groups; the long, relatively narrow basilar membrane responds to higher frequencies. This implies that the ossicular chain of the mammalian ear is not the only factor involved in the high frequency sensitivity of mammals.

It is possible that the straight or slightly curved nature of the avian cochlea has limited the frequency response by limiting the membrane length. In the barn owl and some other birds, the bony canals of the cochlea meet in the midline of the head, that is, in this species a further extension of length is impossible. A relatively longer membrane is needed to achieve the same frequency response in a membrane which is wider. Thus even though the cochlea tends to be longer in larger birds³⁰, the

membrane is wider and the high frequency response of the membrane may be the same. This may be one reason why the birds form such a tightly knit group in the above comparisons. In reptiles the length available for extension has not been fully utilized.

The coiling of the mammalian cochlea permitted great extensions in length of the basilar membrane. Generally, the larger mammals (for example, man) with wider membranes do not have the highly elongated membranes necessary for both high and low frequency hearing. Partly because of this there is some correlation between the weight or size of an animal and its hearing range.

Patterns of Frequency Analysis

Although some selective tuning is present in elliptical membranes^{7,30}, in many ways the shorter reptilian membranes are poor frequency analysers, as outlined later. These membranes are labelled “primitive” reptile membranes on Fig. 6. When a large sample of neurones is obtained from the cochlear nucleus of a mammal or bird, there are more neurones with their characteristic frequency near the best frequency of the audibility curve than at the extremes^{4,10}. This simple distribution is also found in the caiman⁶. In some reptiles, however, the neurones are grouped around some characteristic frequencies, with other frequencies unrepresented by neurones⁷. This would seem to leave “gaps” in the frequency responsiveness of the system at low sound intensities. In some cases, also, the single-neurone tuning curves are most sharply tuned at the characteristic frequencies at which the neurones are grouped. This contrasts with the situation in mammals, birds, geckos and the caiman, for which the sharpness of tuning curves tends to increase with frequency monotonically. Further, the single-neurone tuning curves tend to be simple in shape for the longer membranes, but complex for the shorter ones, showing a number of minor increases in sensitivity at frequencies other than the characteristic frequency. These other frequencies often correspond to frequencies at which the neurones are grouped⁷. All these peculiarities are present only in the responses from short membranes, with no clear morphological gradient along their length. The evidence indicates that these membranes respond well to only a few narrow bands of frequencies, and these responses contain ambiguity.

Those reptiles in which the membrane shows a clear morphological gradient are more mammal-like in their auditory responses. They also resemble the mammals in another important way. As I have noted, an orderly fibre projection from the cochlea preserves the frequency separation in a geometrical fashion on the brain nuclei. Comparable tonotopic organizations are found in the nuclei of the caiman and to some extent the geckos, but not in reptiles with short membranes⁷. This, taken with other evidence outlined above, may indicate that these “advanced” reptilian membranes analyse frequencies in a way similar to those of birds and mammals.

Thus in the evolution of tetrapod hearing systems there were at least two important stages. The first was the elongation of the primitive membrane, such that responses to different frequencies were distributed in an orderly way along the length of the membrane. A longitudinal gradient in width and/or stiffness was necessary for this to occur. This new method of analysis produced a more consistent and less ambiguous frequency analysis. Further, the frequency range could be extended by elongation of the membrane. In the birds, however, this elongation was severely limited by the size of the skull.

A second important stage was the coiling of the cochlea in some mammalian stocks. This allowed much greater extensions in length and consequently the ability to analyse much higher frequencies than any bird or reptile. Coiling probably also had other effects on membrane response³³. Thus in a general way, evolution has led to an increase in sensitivity,

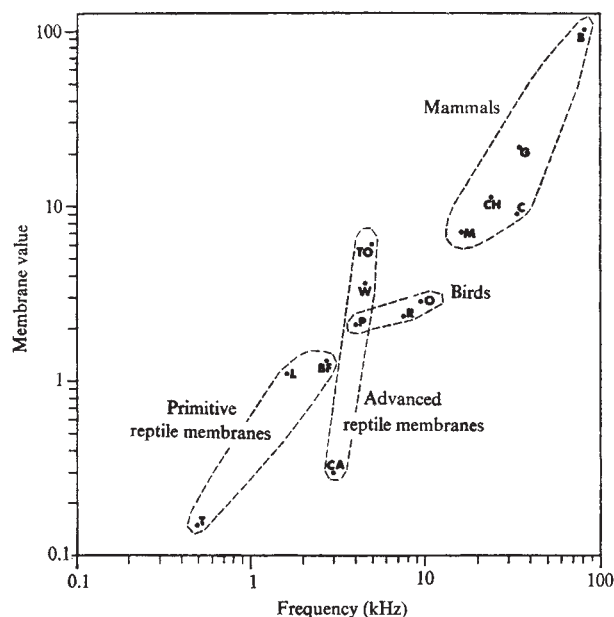


Fig. 6 Correlation between the dimensions of the basilar membrane (see text) and the highest characteristic frequencies of single neurones in different species of reptiles, birds and mammals. Abbreviations: T, box turtle; L, leopard lizard; BF, blue fence lizard; CA, caiman; W, western banded gecko; TO, tokay gecko; P, pigeon; R, robin; O, barn owl; M, man; CH, chinchilla; C, cat; G, guinea-pig; B, bat.

more consistent analysis of different frequencies and higher frequency responses in the vertebrate ear.

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Late Australopithecine from Baringo District, Kenya

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Faunal remains from Chesowanja, a new Pleistocene fossil locality in the Northern Rift Valley of Kenya, include a partial cranium of a late robust australopithecine. Isotopic dates suggest an age of 1.1 to 1.2 million years for the fossiliferous sediments.

CHESOWANJA, a new Pleistocene fossil locality, in the northern Rift Valley of Kenya, lies at 36° 12' E, 0° 39' N, 1.5 km to the west of the track between Mukutan and Chemoigut. It was

discovered by J. C. while mapping the area to the east of Lak Baringo. Faunal remains found include a partial cranium of an australopithecine.

The fossiliferous sediments are contained in an embayment on the west of the Chemoigut sedimentary basin. The basin extends approximately 20 km by 5 km along the foot of the Aruru Escarpment and is about 1,100 m above sea level. Isotopic dates taken from lavas that dammed the basin suggest an age of 1.1–1.2 m.y. for the sediments.

Geological Setting

The Chemoigut basin is in a topographic setting moulded by several phases of volcano-tectonic activity which have been important in the morphological evolution of the Northern Kenya Rift Valley. The following sequence of geological

events led to the establishment of this sedimentary basin. The general disposition of the units involved is shown in Fig. 1.

(a) Mio-Pliocene volcanics: these lavas form a faulted, dissected plateau of considerable relief and are exposed along the eastern and southern margins of the lake basin. The two

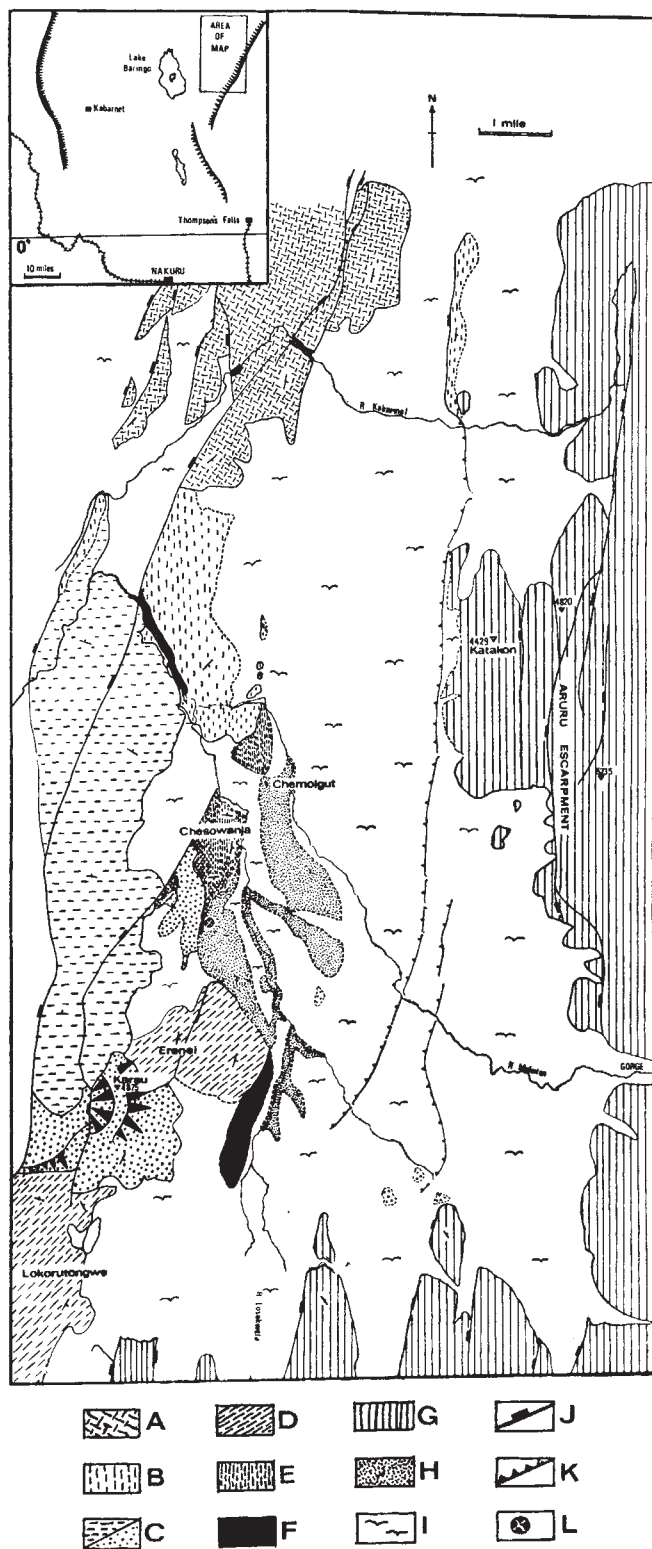


Fig. 1 Simplified geological map of the Chemoigut basin (heights in feet). Key: A, Chepchuk trachyte (date 1.1–1.2 m.y.); B, Lower Mukutan basalt; C, Karau trachytes and tuffs; D, Erinei lavas and Lokurutongwe lavas; E, Chesowanja basalt; F, Lake Hannington type phonolites; G, Plateau phonolites; H, basin sediments; I, alluvium; J, major faults; K, terrace feature; L, fossil locality.

formations present are the Samburu Basalts and the overlying Rumuruti Phonolites^{1,2}. Outcrop of the Samburu Basalts is limited to the face of the Aruru Escarpment and the area along the foot of the scarp to the east of the basin. The lavas comprise a succession of alkali-olivine basalts, basanites, and mugearites; ankaramitic varieties are common. The general form of the lava pile in this area is that of a broad shield which has a maximum thickness in the region of Katakoni, a down-faulted intrusive complex in which dyke swarms and plugs are intruded into steeply dipping agglomerates. Fissures located near the Katakoni centre were probably the chief source of the flows constituting the flanks of the dome. Flows of the Rumuruti Phonolites are of the plateau type, sheet-like and extensive. The phonolites flooded over the basalts and lapped around but did not cover the Katakoni centre. Patterson obtained isotopic ages of 11.0 and 12.0 m.y. for these lavas east of this area³.

(b) Major faulting episode and subsequent erosion interval: during and after the eruption of the Rumuruti Phonolites a major faulting episode occurred. Faults show a typical NNE–SSW trend in this area and the principal downthrows are to the west. The phonolites have been downthrown at least 750 m from the Laikipia Plateau to the foot of the Aruru Escarpment, the fault strips and horst blocks having been back-tilted to the north and east. The northward component of tilting is most severe in fault strips to the west of the Aruru Escarpment, and these have become obscured to the north by later volcanics and lacustrine deposits. This faulting has defined the eastern limits to the lake, and controlled drainage and sedimentation. Dissection of this phonolite plateau was consequent on the faulting. The Mukutan Gorge, located along an east–west fault, is a good example of the intensity of downcutting during this period. The pre-Pleistocene topography of this area must have been one of considerable relief, over which the rivers draining the faulted plateau maintained a westward direction by backcutting and downcutting their channels.

(c) Upper Pliocene–Pleistocene volcanics: a series of Pliocene–Pleistocene eruptions completed the damming and ponding of drainage from the eroded plateau region. Flows presumed equivalent to the Lake Hannington Phonolites² rest with marked unconformity on the eroded Mio-Pliocene volcanics and form a floor to most of the later Pleistocene volcanics. Sources are obscure; the sheet-like nature of the flows suggests eruption from fissures. These flows are overlain by the Chesowanja basalt, a grey doleritic lava which outcrops as a long, low ridge that is assumed to overlie a fissure source. The extent of the flows around the ridge has been obscured by faulting and by lacustrine deposits that abut against it. Probably contemporaneous with this basalt are the small phonolitic trachyte centres of Erinei and Lokurutongwe which are located to the north-east and south of Karau and have been partly covered by Karau tuffs. Karau is a volcanic cone from which was derived most of the tuffaceous material found in the lacustrine sediments. The edifice now stands some 270 m above the surrounding country; the flank deposits consist of crumbly coarse tuffs and welded tuffs both of which contain fragments of lava and glass. An extensive tuff-plain originally surrounded Karau, and pockets of well stratified pumice tuff are found up to 6 miles north of the cone. One of the final phases of Karau involved faulting and breaching of the north wall of the crater. Overlying Karau tuffs in the lower Mukutan Valley is another flow of grey doleritic basalt which came from an unexposed source to the west. More evidence of this second basalt phase is provided by a dyke of doleritic basalt which has been intruded into Karau pumice tuffs south of Chesowanja hill. The final phase of volcanism which influenced the development of the lake basin was the eruption of trachytes from the large central volcano of Chepchuk. These flows probably completed the volcanic barrier to the basin. Although these volcanics represent separate eruptive events there was probably a good deal of overlap in time. A volcanic bomb

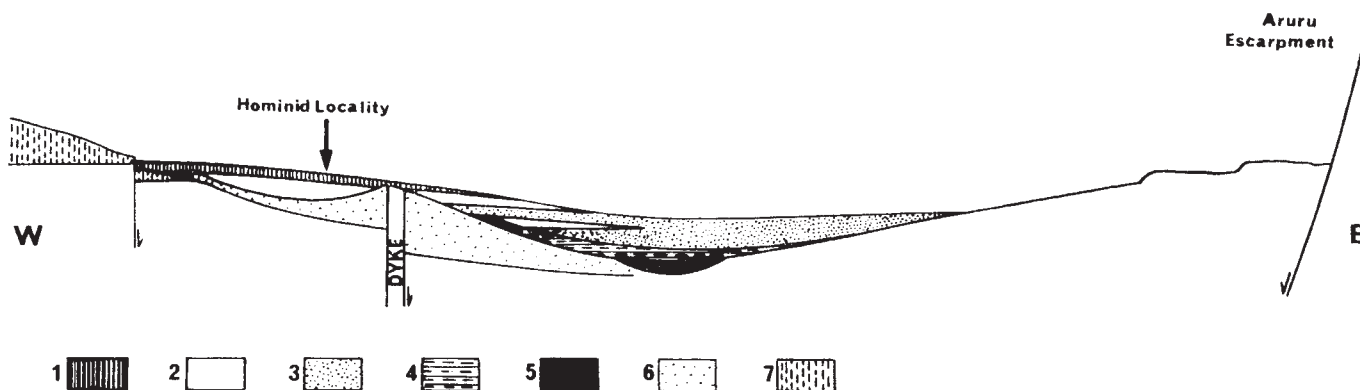


Fig. 2 Schematic east-west section across the Chemoigut basin. Key: 1, Purple marls; 2, brown clay/yellow marls; 3, green silts; 4, tuffaceous silts; 5, tuffaceous conglomerate; 6, Karau pumice tuffs; 7, Chesowanja basalt.

found in the sediments is basaltic although its source is unknown.

(d) Chemoigut Lake Basin: eastern boundary of the basin is a well defined boulder feature developed on outwash deposits from Aruru and Katakun, which extends for several miles before splitting into two north of the River Mukutan. These two features continue southwards, dying out around the inferred southern margin of the lake. The lake was bounded to the south by Miocene volcanics, Karau cone, and Erinei lavas. Northwards, these are succeeded by the Chesowanja basalt, the lower Mukutan basalt and the Chepchuk trachytes. The western limits of the lake are inferred from the distribution of the sediments and the height of the barrier volcanics. At its maximum the lake possibly occupied an elongate area some 20 km long, north-south, by 5 km, east-west.

Lake Sediments

The lake sediments are found chiefly on the west side of the basin in the Losokweta valley-Chesowanja region, and north-east to Chemoigut. Towards the centre of the basin more recent silts and conglomerates obscure the lacustrine sediments, which are seen only in channels that cut the flood plain deposits or as inliers between the various river systems. Farther north dark brown silts and muds derived from the weathering of the basalts around Katakun and in the face of the Aruru Escarpment have similarly obscured the lake sediments of that region. The general sedimentary succession varies throughout the basin. In the upper Losokweta valley sediments are chiefly tuffaceous whereas near Chemoigut tuffaceous horizons are overlain by well sorted calcareous conglomerates. The situation in the Chesowanja embayment is atypical of the lake basin sedimentation as a whole. A maximum thickness of lacustrine sediments here is 18 m and towards the east the deposits thin out. The fossiliferous sediments were deposited in a lagoon formed by the division of the embayment by the basalt dyke (Fig. 2). East of the dyke the succession begins with a fluvial phase and is as follows.

(1) Tuffaceous conglomerate consists of clasts of Rumuruti Phonolite, Karau ignimbrite and reworked tuffs are contained in a fine grey tuffaceous calcitized matrix. The unit outcrops in the Losokweta valley, forming sheet-like or lenticular bodies of variable extent. The unit is less coarse in its upper parts.

(2) Tuffaceous silts are white-weathering red-brown silts of fluvial origin. They contain pale brown pumice and ferruginous concretions.

(3) Green silts which are micaceous and finely laminated are the most constant lithological unit in the succession and mark the onset of lacustrine conditions in the basin. They are cross-bedded in parts and some of the laminae are stained with dendritic manganese. Drifted plant remains are often found, and near the Chesowanja basalt rootlet traces are common. Pale brown structureless tuffs occur in discontinuous lenses.

(4) Brown clay is an intercalation in the green silts in the region of the embayment, where it contains calcareous concretions and minor calcareous conglomeratic bands. To the south the clay becomes more tuffaceous.

(5) Yellow marls, pale and structureless, overlie the green silts in the west.

(6) Purple marls are fine, friable and structureless and contain many rounded calcareous concretions. The concretions have a concentric structure, usually about a nucleus and are of algal origin.

Of these facies the Yellow and Purple marls are the only two to be developed within the lagoon. In addition a conglomerate is well developed as pebble sheets adjacent to the western margin of the dyke. It is highly fossiliferous and transgressive within the Yellow and Purple marls. Most of the fauna was recovered from this unit although some material, including the hominid, came from the Yellow marls.

Potassium-Argon Age Determinations

Age determinations were carried out on samples of separated feldspar and whole rock trachyte from the Chepchuk volcanics

Table 1 Results of Conventional K/Ar Dating

Sample	K ₂ O (%)	Atmospheric contamination (%)	V/M	Age and error (m.y.)
2078 Trachyte. Separated feldspar 50/100	5.13	67.8	1.925×10^{-4}	1.13 ± 0.06
2078 Trachyte. Separated feldspar 50/100	5.13	66.6	2.005×10^{-4}	1.17 ± 0.06
2078 Trachyte. TR 50/70	4.88	66.0	1.980×10^{-4}	1.22 ± 0.06
2078 Trachyte. TR 50/70	4.88	66.1	1.952×10^{-4}	1.20 ± 0.06
2079 Volc. bomb. TR 50/120	2.17	90.6	2.34×10^{-4}	3.24 ± 0.32
2079 Volc. bomb. TR 50/120	2.17	90.3	2.31×10^{-4}	3.20 ± 0.32

$$\lambda_{\alpha} = 0.584 \times 10^{-10} \text{ yr}^{-1}$$

$$\lambda_{\beta} = 4.72 \times 10^{-10} \text{ yr}^{-1}$$

$$V/M, \text{ volume of radiogenic argon-40 (mm}^3 \text{ NTP)/g of sample.}$$

and on the volcanic bomb found in the sediments. Argon extractions were carried out as described by Miller and Brown⁴ and the isotope ratios measured using an omegatron type mass spectrometer⁵.

Results from the whole rock trachyte and associated feldspar are in good agreement within experimental error. These analytical results together with those obtained from the volcanic bomb are given in Table 1. It was hoped that the bomb could give an upper limit to the age of the sediments from which it came, but the potassium-argon results clearly show it to be erratic. In view of the fresh nature of the trachyte, the results are considered to be very close to the true age of extrusion. The extremely altered state of the volcanic bomb would suggest that the average age of 3.22 ± 0.32 m.y. is a poor minimum value.

Possible Sequence of Events

(1) Northwards diversion of predominantly westward drainage was caused by the eruption of the Upper Pliocene/Pleistocene volcanics. This resulted in the base-levelling of the Pliocene river systems and an extensive plain of aggradation was formed in which reworked Karau tuffs were mixed with flood plain silts and pebbles derived largely from the plateau but also from Karau lavas.

(2) Eruption of Chepchuk trachytes formed the final barrier and the lake was initiated.

(3) The lake was of short duration. The greatest development of sediments was around the southern and western margins. They were partly derived from the Karau tuff cone and partly from detritus from the older dissected plateau region. The material of the boulder terraces was derived from outwash fans along the foot of the Aruru Escarpment. Within the lagoon different conditions prevailed, probably as a result of the then submerged basalt dyke which prevented detrital silts and tuffs from entering this area. The fossiliferous conglomerate was developed as a result of local high energy conditions at the dyke-wave interface. The algal concretions are also evidence of shallow water or strongly evaporative conditions within this embayment, the calcium being derived from the nearby Chesowanja basalt.

(4) Drainage of the lake was possibly consequent on regional

faulting and tilting. An overspill channel in the Chepchuk trachytes developed as a gorge which progressively downcut until the lake was completely drained. The Lower Mukutan Gorge, presumed to be formed by backcutting from the fault scarp, has subsequently captured the Losokweta drainage, but it played no part in the draining of the lake.

Palaeontology

A preliminary faunal list is given in Table 2. Plant remains were also found; abundant root casts, especially near the Chesowanja basalt, suggest that the embayment was flanked by reeds and the presence of calcareous algae indicates that the lagoon was warm and shallow. *Bellamya* and catfish are animals tolerant of relatively stagnant water, and such a situation would also be suitable for turtles and crocodiles. This inference about conditions in the lagoon is supported by the well preserved state of the hippos, which presumably died in the water. One skeleton is almost intact. Observations on modern dead hippo have shown that the gases produced by bacterial action after death cause the carcass to float. Subsequent rotting, attack by fish and current action result in individual bones being dispersed over a wide area. Only rapid burial, for which there is no evidence at this locality, or shallow calm conditions would explain the preservation of a more or less complete skeleton. Remains of the terrestrial animals in the assemblage are more fragmentary, though there is preserved intact the greater part of a pig cranium and some complete bovid mandibles. Most of the fossils are covered to a depth of several millimetres with a layer of prismatic calcite.

The Chesowanja area today is one of *Acacia* bushland or bushland thicket with a very sparse understory, which could have supported populations of the animals in the fossil fauna. Modern species of *Equus*, *Ceratotherium*, *Tragelaphus*, *Kobus*, *Connochaetes*, and *Antidorcas* can all be associated with a bushed grassland habitat, and a similar environment seems probable for the fossil representatives and for the extinct primates, proboscideans, giraffe and pigs. Possibly the subsequent change in vegetation has been brought about by human activity in recent times.

Seven of the fourteen identified mammal genera have modern representatives in Africa. Detailed comparisons with other

Table 2 Preliminary Faunal List

MOLLUSCA	Gastropoda		<i>Bellamya</i> sp.
	Bivalvia		Undetermined
PISCES	Siluriformes		Undet.
REPTILIA	Chelonia	Trionychidae	Undet.
	Crocodylia	Pelomedusidae	Undet. cf. <i>Crocodylus niloticus</i> Laurenti
MAMMALIA	Primates	Cercopithecidae	<i>Simopithecus</i> sp. sp. indet.
	Proboscidea	Hominidae	<i>Australopithecus</i> sp.
		Elephantidae	<i>Elephas</i> cf. <i>recki</i> Dietrich
		Deinotheriidae	<i>Deinotherium bozasi</i> Arambourg
	Perissodactyla	Equidae	<i>Equus</i> sp.
		Rhinocerotidae	<i>Ceratotherium</i> sp.
	Artiodactyla	Suidae	<i>Pronotochoerus jacksoni</i> Leakey "Metridiochoerus" sp.
		Hippopotamidae	<i>Hippopotamus</i> cf. <i>gorgops</i> Dietrich
		Giraffidae	Undet.
		Bovidae	<i>Tragelaphus</i> sp.
		Tragelaphini	Undet.
		? Tragelaphini	Undet.
		Bovini	Undet.
		Reduncini	<i>Kobus</i> sp.
		Alcelaphini	? <i>Megalotragus kottwinkeli</i> Schwarz ? <i>Connochaetes</i> sp. Small sp. Smaller sp.
		Antilopini	<i>Antidorcas</i> sp.

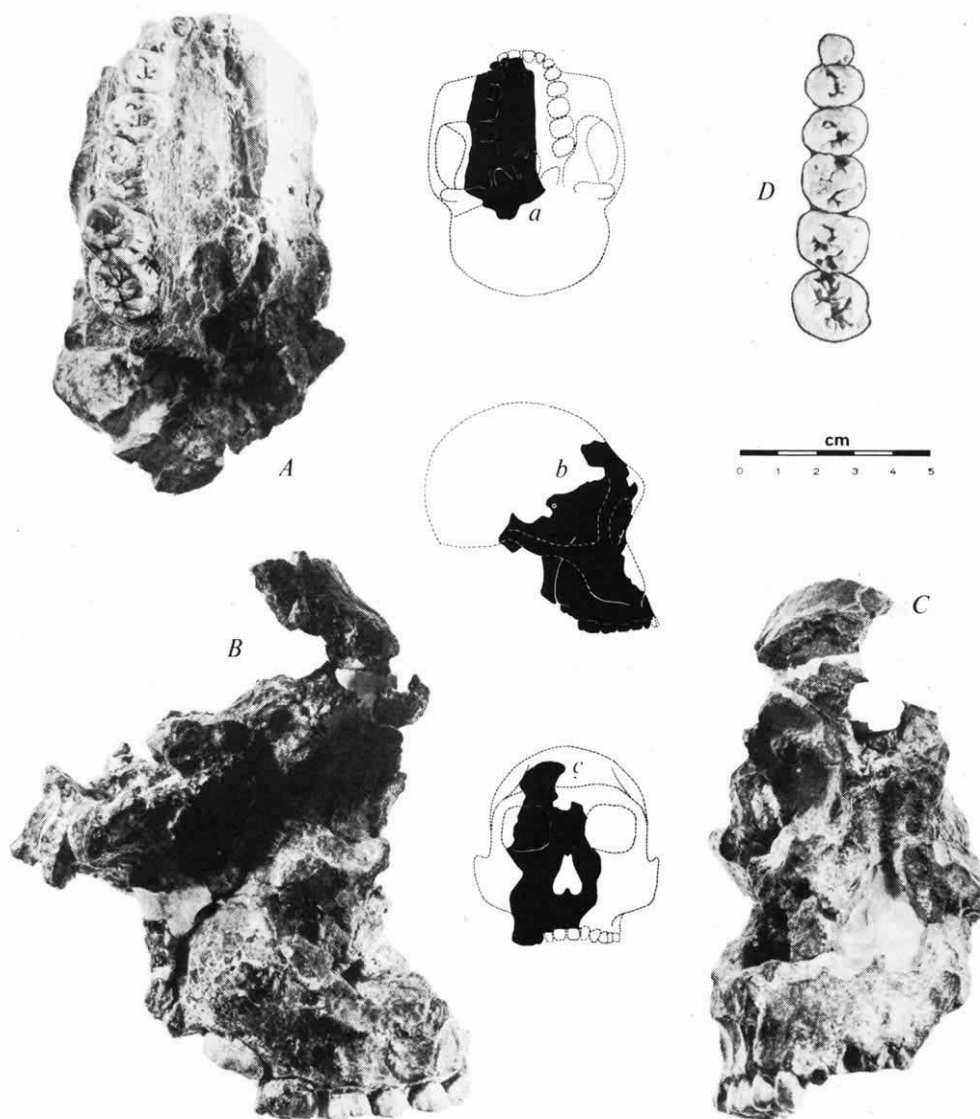


Fig. 3 A, B and C, Palatal, right lateral, and frontal views of KNM-CH-1; with sketches (a, b and c) to indicate the position of the fragment. D, Teeth of KNM-CH-1 with M³ aligned with the occlusal plane.

East African sites are not possible at present, but the mammalian fauna has elements in common with Olduvai Bed II⁶ and Peninj⁷ in Tanzania. *Deinotherium bozasi* occurs at Chesowanja and is the latest known occurrence of the genus (personal communication from J. Harris). It is not found at Peninj or above Olduvai Lower Bed II. The reason for this absence may be that the environments there are different, or the absence may indicate that Chesowanja is equivalent to part of Lower Bed II or to the lacuna between Lower Bed II and Middle Bed II. If this speculation and the Chesowanja dates are correct it would suggest a maximum age for Olduvai Middle Bed II of about 1.2 m.y. Available evidence indicates that the Peninj fauna was contemporary with that of Olduvai Upper Bed II⁷.

Partial Australopithecine Cranium

The specimen (KNM-CH-1) consists of most of the right facial skeleton together with parts of the left facial skeleton, part of the right frontal bone and the right basal part of the calvaria as far posterior as the petrous. The right maxillary teeth from canine to third molar are well preserved. The specimen was found covered by a thick layer of radiating calcite crystals that have now been removed except for those lining the paranasal sinuses, the nasal cavity and part of the right orbit. Distortion is slight and consists chiefly of folding of the right orbital plate, by lateral pressure, and slight flexion of the hypophyseal fossa that is probably associated with this deformation. The midline relationships are still correctly

maintained because a sagittal plane intersects the crista frontalis, the internasal suture, the anterior nasal spine, prosthion, the incisive foramen and the posterior nasal spine. The base of the cranium, especially in the midline, has been cracked and crushed and is the least well preserved part.

Several features of the cranium immediately suggest affinities with *Australopithecus robustus* (Broom, 1938) and *A. boisei* (Leakey, 1959). These are the size and generally massive build of the face, very small canine, massive, buccolingually expanded cheek teeth, the morphology of those teeth, evidence of a prominent glabella and a broad dishd face. Although further comparisons have to be made it seems very likely that the Chesowanja specimen is sampled from a population of evolved robust australopithecines, most likely descended from *A. boisei*. In one or two respects the specimen shows signs of differentiation in a way that is totally unexpected and gives hints of problems in interpretation that might have to be faced in the future.

Apart from the canine and M³ the morphology of the other four teeth can be matched with that of individual teeth from Swartkrans in South Africa⁸. As judged by the alveoli, the incisors were small, like the canine, and these small anterior teeth are overshadowed by progressively larger cheek teeth. It is clear that although the incisors and canines were still a necessary part of the masticatory apparatus, selection had been acting to produce great cheek-tooth area. The alveoli for both medial incisors are present and have mesiodistal diameters of about 7.0 mm. The right lateral incisor is represented only by

the broken root which measures 4.5 mm mediolaterally and 6.0 mm labiolingually. Some dimensions of the teeth are given (Table 3) for comparisons with data given by Tobias⁹ for other australopithecines. With the exception of the canine diameters the other measurements generally fall close to the mean recorded values for *A. robustus* and only the buccolingual diameters fall within the recorded range for *A. africanus*.

Table 3 Measurements and Indices of the Teeth of KNM-CH-1 (in mm)

	Mesiodistal length	Buccolingual breadth	Tooth module	Crown area	Shape index length/breadth
C	8.2	8.4	8.30	68.88	97.6
P ³	10.0	13.7	11.85	137.00	72.9
P ⁴	11.7	15.5	13.60	181.35	75.5
M ¹	14.4	14.8	14.60	213.12	97.3
M ²	15.5	16.0	15.75	248.00	96.9
M ³	17.2	16.5	16.85	283.80	104.2

Details of the tooth morphology will be recorded elsewhere, but as many workers are attempting to interpret australopithecine tooth wear in terms of dietary hypotheses, the following observations on wear may be of interest. The canine, although not much worn, has two occlusal wear surfaces as in Olduvai Hominid 5. The premolars are slightly worn and only the tips of the cusps are faceted. M¹ has a very distinct patch of hollow wear on the protocone that must have been produced when the second deciduous molar was *in situ*, but apart from this patch, wear is not great and the hypocone is almost unworn. M², although fully formed, is unerupted. The absence of marked wear on any tooth is most striking and it may be that the eruption intervals between the various teeth were shortened in these animals to create, in a relatively short time, an efficient, smooth grinding tooth row. In conjunction with this is the development of the curve of Spee—a longitudinal convexity to the tooth row that is considered to be an essential part of the grinding dentition. The shape of the palatal arcade is similar to the condition in the Swartkrans *A. robustus* specimens and the palate itself was fairly deeply excavated (estimated 23.0 mm at the palatine torus).

The nasal region is set in a central facial hollow that is formed by the projecting naso-alveolar clivus, the presumed large glabella and the zygomatic processes of the maxillae. These arise from the alveolar processes very far forwards at a level just behind the canines. These processes not only take off from a very anterior position but also a very inferior one, which would have the effect of aligning the anterior attachment of the superficial fibres of *m. masseter* as tangential to the curve of Spee as possible.

The set of the face on the cranium is very posterior so that the pterygoid plates are set nearly at right angles to the Frankfurt horizontal. The cranial base is crushed in the midline but the posterior position of the molars and the angulation of the pterygoids would argue for the presence of quite a high degree of basicranial flexion. Allied to this setting of the face under the cranium is the fact that postorbital constriction in KNM-CH-1 is not nearly as great as seen in other robust australopithecine specimens. Three features point to this fact: (1) allowing for the folding of the orbital plate the measured half-postorbital width from the midline is great (estimated to be 40.0 mm) and compares, for example, with half-postorbital widths of 35.0 mm for Olduvai Hominid 5 (ref. 9) and 31.0 mm for the recently reported East Rudolf cranium¹⁰; (2) the frontal fragment shows no signs of marked ridges for the attachment of *m. temporalis*, the anterior fibres of which occupy the postorbital notches; and (3) the anterior cranial fossa is long and anteriorly positioned so that the orbital cavity would be immediately under the frontal lobe of the brain and make a marked orbital impression on the endocranial surface—as in *A. africanus* and *Homo* spp.

The supraorbital torus and glabella are missing but examination of the well developed frontal air sinus and the profile of the frontal and nasal bones gives an indication of the great development of both of these features as well as showing that the cranial vault rose steeply behind the torus as in *A. africanus*.

The endocranial aspect of the cranium is interesting. KNM-CH-1 has clearly much bigger anterior and middle cranial fossae than in Olduvai Hominid 5 or as seen in the SK 1585 natural endocranial¹¹—the only robust specimens available for comparison—and much bigger than in *A. africanus* specimens. Quantification of the size difference is difficult because of the fragmentary nature of the specimen, but the test of fitting endocranials into the endocranial surface is most telling. Given the nature of the evidence, it would be unwise to estimate the cranial capacity of the specimen, but if the proportions of the various parts of the endocranium are the same in KNM-CH-1 as in Olduvai Hominid 5 or SK 1585, then the cranial capacity would have been more than 530 cc.

Another specimen of about the same age that may have been part of this late robust australopithecine population in East Africa is the Peninj mandible¹² that occludes well with the Chesowanja cranial fragment. Studies are in progress to show whether, on morphological grounds, the two specimens could or could not be reasonably expected to come from one population. The Peninj mandible also has an extremely small canine and progressively larger cheek teeth¹³.

In summary the specimen seems to be a late robust australopithecine in which some of the more gross features typical of the groups are refined and which has a somewhat confusing amalgam of features. Some features resemble those of *A. robustus* and *A. boisei*, some *A. africanus* and some show trends that have been taken before as typical developments towards *Homo*. The tantalizingly suggestive evidence of enlarged cranial capacity, the tucking of the face under the braincase, either reduction or refinement of the masticatory apparatus such that extremely large anterior bundles of *m. temporalis* were no longer needed are all pointers to development in the lineage of trends towards *Homo* that are, perhaps, demonstrable in other hominid lines. Were the robust australopithecines an over specialized group heading for extinction as Tobias has suggested⁹ or were they still, only one million years ago, an adaptable and viable group whose descendants have yet to be found or to be recognized?

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LETTERS TO NATURE

PHYSICAL SCIENCES

Occultation by Jupiter

AN occultation of β Scorpii (2.^m9) by Jupiter has been predicted to occur on May 13, 1971, at about 1800–2000 UT¹. Although not visible from North or South America or western Europe, the occultation should be visible from the rest of the inhabited world. The results of two previous occultations by outer planets (σ Arietis by Jupiter in 1952² and BD-17° 4388 by Neptune in 1968^{3–6}) have been rather disappointing in that scale heights deduced from them have uncertainties of a factor of 2 or more. There is also considerable uncertainty in the temperatures, so the derived mean molecular weights have not been very useful. In addition there is probably a bias in the procedure of fitting theoretical “light-curves” to the observed data introduced by “spikes” in the light curves which have been attributed to inhomogeneities in the subject atmospheres.

I suggest a different approach which avoids most of the shortcomings of the traditional method and which could lead to an accurate determination of the H_2 : He ratio on Jupiter. This approach makes use of the timing of the “spikes” or intensity anomalies that have been observed and does not depend directly on intensity measurements. Basically I exploit the fact that, because of the very small angles of refraction involved, light of different colours will traverse essentially the same paths through the atmosphere and yet the spacing in time of the “spikes” received will accurately reflect the difference in index of refraction between the two colours. Since the change in refractive index is different for H_2 and He the composition can be determined.

The angle of refraction of a light ray along a path through an atmosphere, θ , is given to a good approximation by

$$\theta = \int_{-\infty}^{\infty} \frac{1}{n(r)} \frac{dn(r)}{dr} dx \approx \int_{-\infty}^{\infty} \frac{dn(r)}{dr} dx$$

Here $n(r)$ is the refractive index of the atmosphere at distance r , the integral is over the path, and we have made the usual assumptions (θ small, spherical symmetry, atmospheric scale height $\ll$ planetary radius and so on). If r_1 is the distance of closest approach and D is the planet–observer distance, we have

$$\frac{I}{I_0} = \frac{1}{1 - D \frac{d}{dr_1} \left(2 \int_{r_1}^{\infty} \frac{r}{\sqrt{r^2 - r_1^2}} (n_0 - 1) d\rho(r) \right)} = \frac{1}{1 - (n_0 - 1) 2D \frac{d}{dr_1} \int_{r_1}^{\infty} \frac{r}{\sqrt{r^2 - r_1^2}} d\rho(r)}$$

I is the observed intensity of the star, I_0 is the preoccultation intensity, n_0 is the index of refraction of the atmosphere at standard density and $\rho(r)$ is the atmospheric density. The time dependence of the observed signal intensity is determined by the factor

$$\frac{d}{dr_1} \int_{r_1}^{\infty} \frac{r}{\sqrt{r^2 - r_1^2}} d\rho(r)$$

Consequently a change in $(n_0 - 1)$ is equivalent to a change in time scale of the received signal.

Fig. 1 shows six calculated theoretical occultation light curves, for H_2 and He atmospheres at each of three wavelengths. An

exponential, isothermal atmosphere was assumed with randomly generated fluctuations up to 5% in refractive index (perhaps due to local heating or cooling, eddy diffusion or stratification). The mean scale height assumed was 20 km, the planetary radius 25,000 km, the planet–observer distance 5 a.u., the speed of the planet’s shadow toward the observer 10 km s^{−1}. The vertical scale of the fluctuations was ~ 2 km. These calculations represent something of a mixture of the outer planets. They are intended to be illustrative only and no attempt was made to fit any particular planet or occultation data. The extent to which occultation data can in practice be inverted to reveal the atmospheric structure which produced it is an interesting question that is beyond the scope of the present note. Table 1 presents data⁷ on the relevant refractive indices.

Table 1 $(n_0 - 1) = A(1 + B/\lambda^2)$ (λ in μm)

	$A \times 10^{-5}$	$B \times 10^{-3}$
H_2	13.58	7.52
He	3.48	2.3
Air	28.71	5.67

The time scales normalized to 6000 Å (that is the time between receipt of two features divided by the corresponding time at 6000 Å) are as follows: H_2 at 3000 Å, 1.061; H_2 at 1500 Å, 1.307; He at 3000 Å, 1.019; He at 1500 Å, 1.095. An adequate quantitative treatment would include higher order terms (particularly at 1500 Å for H_2 where we may be seriously underestimating the differences) but they are unnecessary for purposes of illustration.

Thus if we had measurements at 6000 Å and 3000 Å (in practice groundbased data would have to be obtained slightly longward of 3000 Å, perhaps 3200 Å from a reasonably good site, and the long wavelength could perhaps be selected to correspond to a strong, wide methane absorption band so as to reduce the planetary background) the measured time scale shift should be between 1.8% (pure He atmosphere) and 6.1% (pure H_2). The H_2 : He ratio is obtained by interpolation.

The structure observed in the Neptune occultation lasted about 200 s while the Jupiter data are only available for about one minute after the onset of occultation. Furthermore, there was a systematic noise problem in the latter, possibly because the telescope tracking error was not completely removed: the pre-occultation portion of the trace is almost as

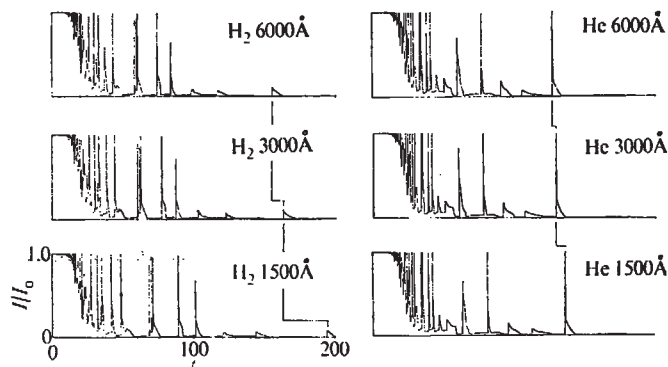


Fig. 1 Representative synthetic light curves for H_2 and He atmospheres on a major planet at three wavelengths. Irregularities are due to random fluctuations superimposed on an exponential atmosphere. Axes are identical for all six curves.

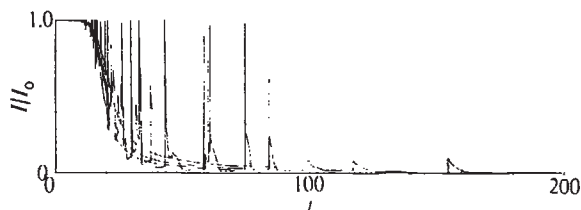


Fig. 2 Three "smooth" light curves (that is, curves calculated for strictly exponential atmospheres) superimposed on the 6000 Å H_2 synthetic light curve from Fig. 1. The uppermost smooth curve is for a scale height of 20 km. The scale height decreases by a factor of $10^{-0.1}$ in each succeeding lower curve. Sharpness of the spikes would be degraded in a real situation because of the time resolution of the instrument.

irregular as the post-occultation portion. Fortunately, photographic observations by independent workers (referred to in ref. 2) confirmed the existence of "structure" in this occultation. It is possible that the spikes produced by Jupiter are inherently smoother than those produced by Neptune in which case it would be more difficult to determine the $H_2 : He$ ratio. The occultation by β Scorpii is expected to be near-grazing so that long immersion and emersion periods are available. If sharp intensity fluctuations can be observed for 200 s, a 4% change in time scale corresponds to 8 s, which seems easily within experimental capability. The neglect of trace species in the model can be justified on the basis that the change of refractive index for methane, the most likely trace species, is not greatly different from the expected measured values (this method is certainly less sensitive to the presence of CH_4 than the traditional scale height method which weights each species by its molecular weight).

Further refinements of the technique are possible. If the measurements could be obtained from above the atmosphere the 1250–1700 Å region could be studied; Jupiter is very nearly black in this region which, again, would reduce the planetary background. Also, the time scale shifts are much larger, and even quite broad fluctuations should be adequate. While I have tacitly considered refractive index departures from an exponential atmosphere as causing the intensity fluctuation, the same effect would be caused by attenuation due to, for example, a Jovian analogue of noctilucent clouds. The shape of the spikes would be different but the same principles would apply. Finally, I emphasize that the presence of sharp structure in occultation light curves cannot simply be ignored in the traditional approach of determining scale height. Fig. 2 illustrates several theoretical fits (with scale heights 20 km, $20 \times 10^{-0.1}$ km and $20 \times 10^{-0.2}$ km) to a synthetic light curve. It has none of the statistical and systematic instrumental noise that would be present in a real occultation curve and the fit includes the further simplification that the zero and pre-occultation signal levels are known. The only degree of freedom is in the fitting of the zero of time. In the past the presence of structure has been ignored⁶. Adopting this criterion the best fit would seem to be the middle or lower curve of Fig. 2, although the upper curve was used to derive, by superposition of fluctuations, the synthetic light curve. A least-squares type fit directly to the data would probably have erred in the other direction.

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Is There a Black Hole in ϵ Aurigae?

THE suggestion has been made^{1,2} that the secondary component of the eclipsing binary system ϵ Aurigae is a black hole, the result of a stellar implosion. Current interest in the physics of black holes³ and the research by Wilson⁴ on the dynamics of the supernova phenomenon, which indicates that collapsed objects may be quite common in the galaxy, make Cameron's¹ suggestion particularly intriguing.

We wish to reconsider briefly the observational evidence about ϵ Aurigae, in particular the results obtained since the 1955–1957 eclipse. We believe that some features of models recently proposed are incompatible with the observational data now available.

The reasoning of Cameron¹ and Stothers² proceeds as follows. (1) The primary F supergiant is in a post-main-sequence stage of evolution. (2) As a consequence of (1), the secondary must originally have been the more massive component of the system; it must have evolved faster and is now observed as a black hole after losing a considerable fraction of its mass as a supergiant. Statement (1) is probably correct. But it does not follow that the secondary was originally the more massive component. We believe that the observational evidence indicates that the secondary is the less massive star of the system, and that mass transfer from the primary to the secondary is going on at present, resulting in the formation of a disk of matter about the secondary.

The most complete spectroscopic orbit solution is that due to Morris⁵, based on radial velocities obtained at Potsdam, Yerkes, Mount Wilson and Victoria. The light and velocity curves are insufficient to determine the orbit uniquely, for neither the inclination of the orbit nor the mass ratio M_p/M_s are known (where M_p and M_s are masses of the primary and secondary respectively). Fig. 1 summarizes the results in the mass-luminosity plane for a range of inclinations (75° – 90°) and mass ratios (0.5–2.0). The dashed line on the diagram is the mass-luminosity law for supergiants⁶. The bolometric correction for the F 2 primary is small and was taken to be 0.^m1. An independent estimate of the distance of ϵ Aurigae will yield the solution of the system.

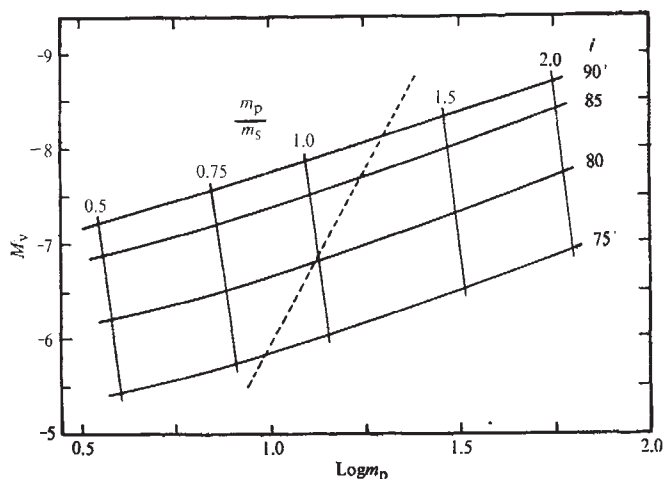


Fig. 1 Possible solutions for the system of ϵ Aurigae in the mass-luminosity plane. The dashed line is the mass-luminosity law for supergiants due to Stothers.

Stothers² has argued that ϵ Aurigae may belong to the stellar association Aur OBI, thus deriving for ϵ Aurigae the absolute visual magnitude $M_v \simeq -8.7$. With this value of M_v , Fig. 1 suggests that the primary is grossly underluminous for its mass, and the orbit yields

$$M_p = 50 M_\odot, M_s = 24 M_\odot, \text{ for } M_p/M_s = 2.1 \text{ and } i = 90^\circ$$

for the case which deviates least from Stothers' mass-luminosity law.

Morris^{6,7} estimate yields lower masses and a somewhat more consistent picture. On the basis of the strengths of the interstellar lines, as calibrated by Beals and Oke⁹, and of the astrometric orbit obtained by Strand¹⁰, he obtains a fainter absolute magnitude for the system ($M_v \simeq -7.5$) and therefore lower masses for the components, that is

$$M_p = 17 M_\odot, M_s = 14 M_\odot, \text{ for } M_p/M_s = 1.2 \text{ and } i = 83^\circ$$

An absolute magnitude fainter than -7.5 is ruled out by Strand's observations. Note also that the value of $i = 83^\circ$ is more consistent with the inclination $i = 73^\circ$ measured by Strand.

We wish to emphasize that both Stothers' distance modulus and that of Morris yield a mass ratio greater than unity. Furthermore, because the primary supergiant is observed to be losing mass^{6,11}, the mass ratio now measured must be lower than that of the original system, barring, of course, mass loss from the secondary, for which we have no evidence.

Using an observation by Mitchell¹⁵ of a large infrared excess for ϵ Aurigae at $9 \mu\text{m}$, Cameron discussed the existence of a large disk of particles radiating in the infrared while spiralling into the secondary, assumed to be a black hole. Further observations by Low and Mitchell¹⁴, however, showed Mitchell's original result to be spurious.

Three lines of evidence support the hypothesis of mass transfer from the primary to the secondary: (a) The line profiles of the F star outside eclipse are variable, indicating instability in the outer atmosphere of the F star⁶. (b) The low excitation lines in the F star spectrum are anomalously strong¹¹, which may be interpreted as evidence for an extended outer atmosphere. (c) The profile of H_α , which is usually in emission, undergoes a series of complicated changes during eclipse¹⁸, which can be interpreted to show that the emission arises from collisional excitation of the gas at the inner Lagrangian point of the system⁶.

The secondary star is embedded in a disk of material which is responsible for the observed eclipses of the F supergiant^{7,12}. The integrated colour of the system remains unaltered during the eclipse, implying that the disk must be either opaque to the radiation from the P star^{6,7}, or composed of particles large compared with the wavelength of light¹². As described below, there is evidence for the disk having a photospheric region with a temperature around 4,000 K, so it must be composed not of large grains but of hot gas, being fully opaque and blocking half the light from the primary at minimum by obscuring half the visible hemisphere of the primary.

Shell absorption lines are observed in the spectrum during ingress and egress, caused by absorption of the light from the F star by the outer regions of the disk, which are optically thin. The temperature in this region has been determined, using curve of growth analyses, independently by Hack¹³ and Morris⁶. Both authors find the neutral iron atoms to be excited by a dilute radiation field at a temperature near 4,000 K. The dilution factor, found by both to be near 0.1, was measured by comparison of transitions from states with metastable and non-metastable lower levels. The geometrical dilution factor for the F star radiation field is about 2×10^{-4} in the vicinity of the secondary¹⁷. This indicates that the outer part of the disk is excited not by the F star but by its own photosphere, which has a temperature of about 4,000 K. The disk must therefore be heated from inside.

Assuming a radiating area for the photosphere of the disk at 4,000 K twice that of the F supergiant (the radius of the disk is known to be approximately 5.5 times that of the F star),

the disk will be 2.3 magnitudes fainter than the F star in the visual, and therefore very difficult to observe outside eclipse. This is in agreement with the small infrared colour excess $E(V-L) = 0.2$ observed by Low and Mitchell¹⁴ and with the lack of infrared colour excess at $9 \mu\text{m}$.

The presence of an opaque disk around the secondary prevents us from observing it directly. The only observational grasp of the secondary we have is that when the shell iron lines disappear, as mid-eclipse is approached, the shell hydrogen absorption lines are enhanced and broadened¹⁸, suggesting the presence of a hot star the radiation of which escapes normally to the orbital plane, through the thinner regions of the disk. The indirect evidence of the secondary we have is the mass, about $14 M_\odot$, and the requirement that it be capable of heating the outer regions of the disk to 4,000 K. These observational constraints are all satisfied by an ordinary main sequence O star.

It is legitimate to inquire whether the secondary could be a black hole, because the presence of the disk nullifies the evidence of Trimble and Thorne⁵ that the secondary is not a black hole. It is conceivable that matter falling from the inner regions of the disk into the black hole could release energy which would satisfy the observational constraints. But until a better understanding of the interaction of ordinary matter with black holes is reached, we shall favour the simpler interpretation.

We believe that when all the relevant data on ϵ Aurigae are taken into account: (1) there is no compelling evidence for a black hole in the system, and (2) the system can best be described by the following model, presented by Morris⁶.

The system originated on the main sequence as a pair of O -type stars, probably with a mass ratio in excess of the currently derived ratio of 1–2. At present, the more massive star has evolved into an F supergiant and is undergoing mass ejection, while the less massive star has not moved far from the main sequence. Part of the mass ejected by the F star remains around it, while part passes through the inner Lagrangian point and forms a disk around the less massive star. The inner region of the disk is totally ionized by the central star; the intermediate region is heated sufficiently to become virtually opaque near the orbital plane; and the outermost zone remains transparent, and produces the shell spectrum obtained during eclipse.

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Neutrons as an Energy Source for the Solar Corona

THE energy source of the solar corona continues to be a problem and the acoustical theory of Sturrock¹ has not surmounted all objections². But there are several reasons why the exothermic decay in the corona of a neutral particle such as the neutron is a very attractive possibility: the immense specific energy required to reach temperatures of 10^6 K, the retention of fast charged particles by the Sun's magnetic field, and the problem of accounting for the high coronal temperature near the relatively low temperature photosphere.

The neutron decays spontaneously according to the scheme

$$n = p + \bar{\nu} + \bar{e} + 0.78 \text{ MeV}$$

with a mean life of 1010 ± 25 s. On the average the decay electrons carry 0.39 MeV, which they will lose after successive collisions with the coronal gas, so heating the corona. Even the reaction protons are born with a temperature of 2×10^6 K. Thus the neutron process has an energy scale comparable with that of the corona.

The time scale of the reaction is also comparable with the travel time across the corona. The scale of the corona is about 10^{11} cm. Neutrons, to leave the Sun and decay in the mean range, would need an initial speed of 10^8 cm s⁻¹, and an initial energy of 5 keV, which is substantially below the energy delivered to them by most neutron producing reactions, and would allow for energy loss during escape by diffusion from a considerable depth in the solar atmosphere.

Finally, the corona generally exhibits a brush-like structure developed along straight, largely radial lines which implies a manifold of neutral rays, originating sporadically in rapidly changing localities of the Sun's surface. Observations have suggested a velocity for this material of about 10^8 cm s⁻¹, which is precisely the scale speed required.

Decaying exponentially in time over their path outward, most of the neutrons would decay at the base of the corona, which explains the sudden rise of temperature from the chromosphere to the corona. The exponential character of the decay can be modified almost at will by varying the depth in the photosphere of the neutron-producing reaction, and hence changing the distribution of outbound neutrons with respect to kinetic energy.

In sum, as a transfer agent for the coronal energy, the neutron is an ideal entity.

The remaining element in a neutron model, the mechanism of the neutron release in the photosphere, is not so easy to pinpoint. Two *a priori* possibilities have occurred to us: sub-surface thermonuclear reactions and production by flares. We believe that the flare mechanism is the more probable, although greater knowledge of flares will be needed before it can be accepted.

In analysing the neutron model of the solar corona, we found that neutrons alone cannot account for the solar wind, or the corona would be vastly hotter than it is. Therefore a mixed flux of neutrons, protons, and electrons must originate at the base of the corona. We assume that the corona is an electrically neutral plasma, and accept the fluid dynamical analysis of the corona and solar wind³, proposing only a modification in the energy source. Therefore we consider only the equation of energy balance

$$\Sigma_i \left\{ \nabla \cdot n_i v_i \left(\frac{1}{2} m_i v_i^2 + w_i - \frac{m_i GM}{r} \right) \right\} + \nabla \cdot \frac{n_3 v_3 E_D}{2} - L_R - L_C = 0$$

where $i=1,2,3$ refers to electron, proton and neutron respectively, n is the density, m is mass and $w_i = 5/2 k T_i$. E_D is the decay energy of the neutron, and L_R and L_C are the loss of energy due to conduction and radiation.

To estimate the neutron flux required at the base of the corona, we equate the total energy reaching the base to the total energy lost by the corona through a sphere at the radius R_e of the Earth's orbit.

$$\Sigma_i \left\{ \frac{1}{2} m_i v_i^3 n_i + \frac{5}{2} n_i v_i k T_i + \frac{n_3 v_3 E_D}{2} - \frac{n_i m_i v_i GM}{R_\odot} \right\} R_\odot^2 = \Sigma_i \left\{ \frac{1}{2} m_i v_i^3 n'_i + \frac{5}{2} n'_i v'_i k T'_i - \frac{n'_i m_i v'_i GM}{R_e} \right\} R_e^2 + L_R + L_C$$

$R_\odot$ is the radius of the Sun, and n'_i , v'_i , T'_i are the density, velocity and temperature at R_e . Assuming that all neutrons decay in the corona, substitution of a flux of 6×10^8 protons cm⁻² s⁻¹ at R_e , 10^{13} protons cm⁻² s⁻¹ at $R_\odot$, $L_R \approx 6 \times 10^{27}$ erg s⁻¹, and $L_C \approx 10^{27}$ erg s⁻¹ leads to a total neutron flux at the base of the corona of

$$n_3 v_3 \approx 1.7 \times 10^{11} \text{ neutrons cm}^{-2} \text{ s}^{-1}.$$

The dominant terms in the computation proves to be

$$\frac{n_3 v_3 E_D}{2}$$

and $L_R + L_C$.

Qualitatively the neutron hypothesis is attractive; it is independent of the internal solar source of the neutrons. But, inability to discover any suitable source would be the eventual undoing of the hypothesis. Therefore we have given considerable attention to the problem with few positive results, although certain mechanisms can definitely be eliminated.

Before examining these results, it is interesting to note that numerous investigators⁴⁻⁹ have searched for a neutron flux at the Earth, and have quoted numbers for this flux which vary from upper limits to definite values. In every case, these numbers compute back to a solar surface flux of the order of the 10^{11} cm² s⁻¹ we require. This is only suggestive of the level of neutron processes which might well be overlooked, however, because the neutrons which we require for the coronal heating would have a decay factor of about 10^{-50} against their reaching the Earth.

We have examined two mechanisms, each with its peculiar advantages and disadvantages. The mechanisms are a sub-surface thermonuclear process, and a surface electrical process.

Burbidge *et al.*¹⁰ have discussed numerous processes in stellar interiors which can lead to an outward neutron flux. In a star with significant hydrogen content such as the Sun, this flux will be captured largely by protons to form deuterium. The deuterium will react by the D-D reaction as long as the local temperature remains high enough, releasing further neutrons, which will on recapture by protons propagate the deuterium concentration outward. A residual concentration of deuterium will thus build up in the colder outer layers of the Sun over its lifetime. De Jager¹¹ formerly suggested 1.4×10^{-4} for the concentration ratio D/H, but a recent search¹² has indicated a value less than 4×10^{-5} . On the other hand, the terrestrial concentration of 2×10^{-4} might well be a better measure of the terminal condition in the colder regions of the Sun, and because the barometric formula would favour an increase downward from the upper layers of the photosphere, the discrepancy with the low photospheric value could be explained.

Whether or not neutrons actually escape through the cool photosphere from the D-D reaction zone in the Sun depends on whether the temperature of the Sun in these outer layers is correctly assessed by current theory, which implies that the reaction zone at which 2×10^{11} neutrons cm⁻² s⁻¹ are available lies at 0.08 solar radii below the surface, while escaping neutrons of 5 keV energy could only be expected to have originated from a depth of 0.0005 $R_\odot$, given an initial energy of 2.4 MeV. On the whole it seems unlikely that even convective turbulence can bring hot reacting materials close enough to the surface to make this mechanism viable.

The surface of the Sun is clearly a region of vigorous electrodynamic activity. The pinch in deuterium has been observed to be a source of neutrons resulting, when thermonuclear temperatures were not achieved, from particle accelerations by moving current sheets¹³. Solar flares are well known to be a source of fast charged particles, and it is tempting to suggest that they are gigantic pinches in short circuit discharges taking place across homopolar generators, provided by turbulent convective vortices turning in the solar magnetic field. These could range in size from small scale turbulence to sunspots, but in general we believe them to be submerged at some depth below the photosphere.

There are numerous reactions which might take place in these circumstances. Leading possibilities for study are $D(d,n)He^3$; $Li^7(p,n)Be^7$; $N^{14}(p,n)O^{14}$. The latter two produce γ rays of 0.44 and 2.31 MeV respectively, and no such γ ray fluxes have been detected at Earth. Although at first sight it might be possible that the reaction occurred at a depth which would shield the Earth from the γ rays, this proves to be an order of magnitude too deep for the escape of neutrons with a residual 5 keV of energy. We are left with the D-D reaction. As noted, the reaction zone must lie at a depth where the overlying mass is about 2 g cm^{-2} , to provide proper neutron energy reduction. This is a depth of roughly $0.0005 R_\odot$, and the hydrogen concentration is $1.5 \times 10^{17} \text{ atoms cm}^{-3}$ (it is interesting to note that this is in the optimum initial pressure range for pinch neutron production¹³). We obtained the value 2 g cm^{-2} by summing the overlying particles encountered in successive free paths on which the energy was increased by a factor of 2 each time between 5 keV and 2.4 MeV, noting the change of cross section with energy. A better result can be obtained by solving Fermi's age equation, but this is not necessary for our purposes.

We assume a cylindrical pre-pinch volume about twenty times as long as in radius r . In the laboratory pinch, only a few neutrons are released before the complete compression takes place. The reduction in radius which might occur can be adjusted almost at will in the present state of our knowledge, but for a strong shock in an ideal gas the factor has been shown¹⁴ to be

$$\left(\frac{\gamma+1}{\gamma-1}\right)^2 = 16.$$

The compression by 16 in radius will increase the initial particle density by 256. Even if the magnetic piston is leaky, the final result is not greatly altered, except that temperature of the pinched gas is increased. The total number of reactions in one pinch will be

$$20\pi \frac{r^3}{256} \sigma v (4 \times 10^{-5} \times 256 N_H)^2$$

where we use Kinman's value of the D/H ratio, σ is the reaction cross section, and v is the deuteron velocity.

It is more difficult to estimate the number of solar reaction zones and the duty cycle of each, but variations in these estimates do not greatly affect the outcome. In our model the reaction zones should match the evidences of turbulence in the Sun, which range in size from sunspots to granules. We choose the latter phenomenon as most indicative of the quiet Sun's behaviour, and place a figure of 10^7 on the number of cells at any given instant. Observation has shown the interval between luminous flare-ups in granules to be of order 10^3 s . We might reasonably assume a neutron-active period of 0.1 s, giving a duty cycle of 10^{-4} . The reasoning behind this is that, both in the laboratory and on the Sun, collapse velocities of $3 \times 10^7 \text{ cm s}^{-1}$ have been observed, and in laboratory pinches the activity period is subsequent to, but of comparable duration with, the convergence time of the current sheet. We are referring to an average time, and larger discharges may involve longer times. The neutron theory of the corona requires 10^{34}

neutrons s^{-1} from the entire Sun, so we reach the condition

$$r^3 \sigma v M = 2 \times 10^4.$$

Bernstein's experiments¹³ showed an excess neutron energy of 300 keV from the pinch reaction. This implied that deuteron energies of 600 keV were available. Use of such a large energy proves to make our active regions far too small to be reasonable. We can therefore easily adjust our parameters to fit such things as a less energetic pinch, a lower compression, a leaky piston or even less deuterium.

If we now assume that the lengthwise dimension of our pinch reaction zone spans the radius of a granule, $r \approx 2.5 \times 10^6 \text{ cm}$. Then if $M \approx 10^3$, σv must be 1.5×10^{-18} . Whether we calculate σv on a basis of monoenergetic particles, or assume a thermal distribution the energy required to produce this value of σv is about 5 keV. Compression alone would only achieve 10% of this temperature. The balance must come from electrical preheating of the gas.

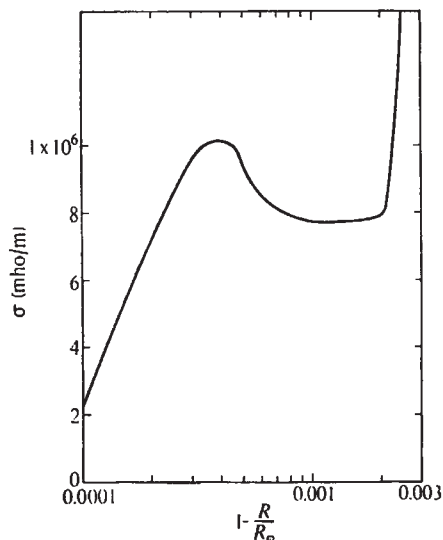


Fig. 1 Electrical conductivity (calculated) of the solar atmosphere as a function of depth, showing insulating layer.

We must now ask whether the general photospheric heating accompanying this process is excessive. Giving all particles, not deuterons alone, the energy just calculated, we find that the power delivered to the photosphere with the constants chosen is only $10^{33} \text{ erg s}^{-1}$, about 10% of $L_\odot$. Even this figure can be quickly and easily adjusted in either direction by large factors, depending on choices of r , M , N_H , and D/H , a fact which may give assistance to any astrophysicists who search for an additional mode of energy transport through deep photospheres.

The energy of 10^{30} erg required to activate a pinch in a volume this large does not originate in the medium lying at the same level as the pinch itself. It must therefore be deeper, and represents an additional mode of bridging the energy transport gap between the deep photosphere and the surface. For the model shown in Fig. 1, the area over which the energy is derived is $\pi(20r)^2$, yielding a necessary value of $10^{14} \text{ erg cm}^{-2}$ of projected area for the generator. Such a value of total energy above 1 cm^2 is not reached in the Sun until a depth of $0.003 R_\odot$, although it speedily increases, and by $0.012 R_\odot$ is 10^2 times this value. This result, however, is of vital importance to the homopolar generator theory, since $0.003 R_\odot$ is precisely the depth at which the electrical conductivity of the solar medium increases quite abruptly to very high values because the hydrogen is approaching full ionization. It is even more remarkable that between this point and the layer where we expect the pinch reactions to occur there is a minimum in the electrical conductivity because the metals have become completely ionized, but hydrogen is only slightly ionized so that

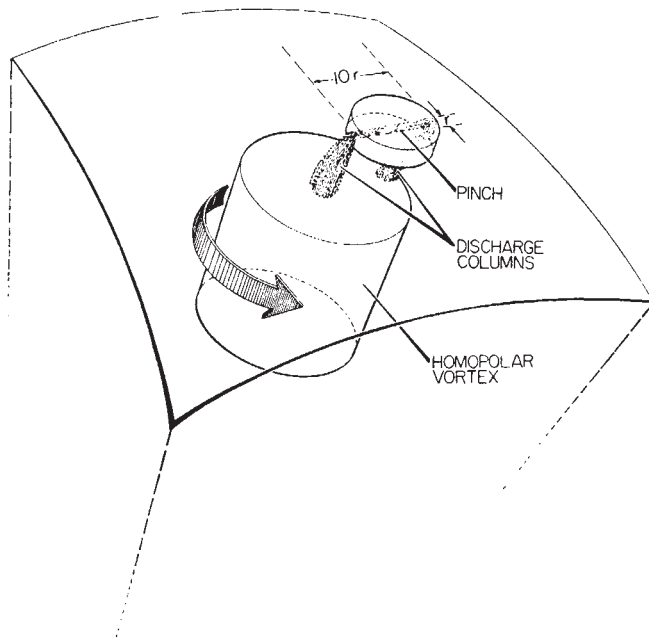


Fig. 2 A possible model of EMF generation in the lower photosphere to achieve insulating layer breakdown followed by pinching discharges through the chromosphere.

the cross section which governs electron mobility is the elastic cross section of the neutral hydrogen atom. This has the striking consequence that there is a maximum in the conductivity precisely at $0.0005 R_{\odot}$, the level at which the pinch reactions must take place in our model. This leaves us with a model of homopolar generator vortices extending from $0.003 R_{\odot}$ down to $0.01 R_{\odot}$, which generate enough EMF to break the mildly insulating layer around $0.001 R_{\odot}$, and produce discharges along the layer at $0.0005 R_{\odot}$.

There is no very clear-cut way to determine the generator EMF needed to produce the discharge we desire. Our estimate of this quantity is suggested by taking the breakdown field to be that one which can build up enough ionization to bridge the conductivity gap. This will be given by

$$\sigma E^2 > e \Phi_i (\beta - \alpha n_e) n_e + \frac{3m}{M} k (T_e - T_0) \nu n_e$$

Here σ is conductivity, E is electric intensity, e is electron charge, Φ_i is ionization potential, β is ionization rate, α is recombination rate, n_e is electron concentration, m is electron mass, M is hydrogen atomic mass, T_e is electron temperature, T_0 is local gas temperature, and ν is the elastic collision rate. In the absence of a field $T_e = T_0$, and $\beta = \alpha n_e$. A rise in electron temperature will increase β and decrease α , so we expand the right-hand side for a small temperature change while neglecting any change in α , inclusion of which would improve our argument. Then

$$\sigma E^2 > e \Phi_i n_e \frac{\partial \beta}{\partial T_e} \Delta T_e + \frac{3m}{M} k \nu n_e \Delta T_e$$

The production of electrons is governed by

$$\frac{\partial n_e}{\partial t} = (\beta - \alpha n_e) n_e$$

where in equilibrium $\frac{\partial n_e}{\partial t} = 0$.

For the change in n_e accompanying our increased ΔT_e in the presence of E , we can write

$$\frac{\partial \Delta n_e}{\partial t} = n_e \frac{\partial \beta}{\partial T_e} \Delta T_e$$

We wish to make a 10% change in n_e in about one second, at a temperature of 2×10^4 K, where

$$\frac{\partial \beta}{\partial T_e}$$

is 40, so we require a temperature increase of 2.5×10^{-3} K. Returning to the equation for E , the principal contribution is from the elastic loss term, and we find $E = 0.3$ V m $^{-1}$. Since we imagine the overall discharge path to be about 3×10^6 m in length, the EMF of our source would be 10^6 V. For a homopolar (Faraday) generator, turning in a magnetic field of 30 gauss, the modest rotation rate of 6×10^{-6} radians per second is required. For a linearly moving volume of plasma, the linear velocity needed would be 300 m s $^{-1}$. These velocities are quite consistent with those observed in sunspot flow fields¹⁵.

It is scientifically distasteful to propose two unconfirmed mechanisms simultaneously in support of each other. Nevertheless, the complete picture seems reasonably satisfying at many points. The great variety of rapid fluctuation phenomena on the Sun can be understood easily if electrical processes are involved. When one considers that the discharges can range in size, in intensity, and in depth, and that not all of them need be accompanied by pinches, there is ample latitude for the morphological differences which pass under the many names of sunspots, flares, plages, granules, flocculi, and so on. A great flare such as 1 September 1959¹⁶ clearly permits interpretation as a pinch which breaks up into sausage and spiral instabilities, and goes over into an outward bound cylindrical shock wave, vanishing on its upper face because there is no matter there, and on its lower face because of the rapid damping of its speed of propagation when entering a medium of exponentially increasing density, thus leaving only the two sidewise moving shocks propagating essentially in a flat channel. Finally, prominences accompanying flares and sunspots can arise from the well known violent extrusion of plasma from the ends of the pinch.

One additional puzzle might also be solved by this theory. The unexpectedly low surface concentration of deuterium in the Sun can be explained if it is being burned as fast as it is diffusing out of the solar interior.

Whatever the fate of the homopolar generator theory of neutron production, neutron heating of the corona possesses advantages which make continued thought about possible surface sources of neutrons of great importance.

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Permafrost at the Summit of Mount Fuji, Japan

THE existence of permafrost at the summit of Mount Fuji, Japan ($35^{\circ} 21' \text{ N}$, $138^{\circ} 44' \text{ E}$, 3,776.3 m above sea level), was suspected by some of the staff members of the Japan Meteorological Agency who work at the Fuji-san Weather Station located at the summit, and also by some of the workers who constructed the station building (personal communication from I. Fujimura). But no official report has previously been published because the observation of permafrost was made only within the small area where the building was constructed. Now, observations on the distribution of permafrost at the summit have been made, during the period from July 31 to August 7, 1970, when the air temperature at the summit is near the maximum. We report here the first definite detection of permafrost in Japan.

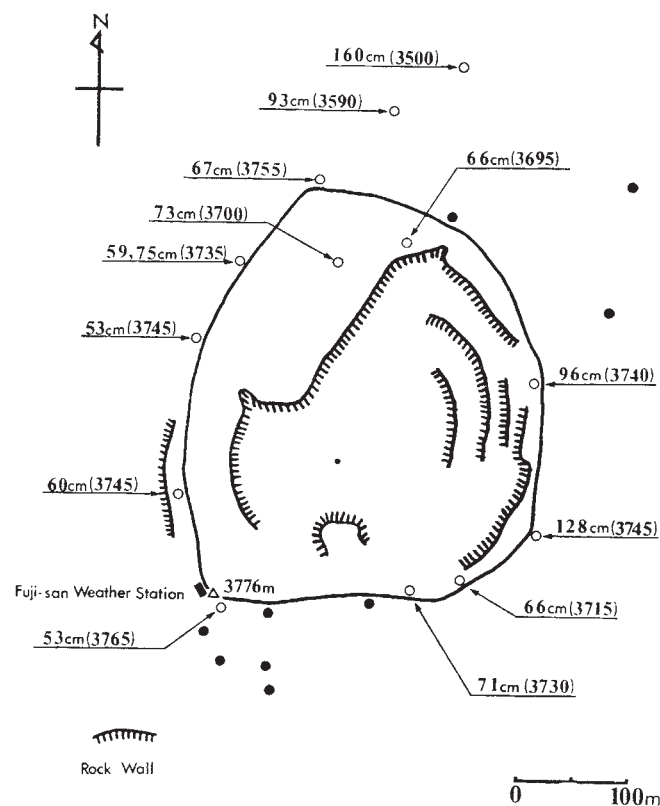


Fig. 1 Depth to the permafrost table around the crater rim at the summit of Mount Fuji (July 31–August 7, 1970).

The depth to the permafrost table was measured by sounding the hardness of the surface layers with an iron bar, the hardness changing discontinuously from an unfrozen layer to a frozen layer. To check the depth obtained by this method, we dug holes deep enough to see the frozen rocks at several points where the soundings were made. The observational results are summarized in Fig. 1, which is a sketch of the crater rim of the summit, the highest point being shown by a triangle at the south-west corner of the crater. The observation points are shown by circles: a white circle indicates a place where permafrost was observed, and a black one a place where it was not. The depth to the permafrost table is shown in cm along the arrows pointing towards the white circles. The altitude of each observation point is shown in m in parentheses.

The existence of permafrost at the summit of Mount Fuji is in agreement with a comparison of climatic conditions at the summit with the relation between the distribution of permafrost and air temperature in Canada and the USSR¹. At the summit of Mount Fuji the annual mean air temperature

is -6.6° C , and the total of the daily average temperatures below 0° C ("freezing index") is 2,810 degree days, while the total of the daily average temperatures above 0° C ("thawing index") is 390 degree days. The average of the maximum depth of snow cover is 84 cm.

As Fig. 1 shows, permafrost was not found on the south facing slope. Brown² reported east–west oriented valleys in which there is permafrost on the north facing slope, but not on the opposite south facing slope in the Cordillera in British Columbia. He explained this difference as due to variations in net radiation on each slope; the absence of permafrost on the south facing slope of Mount Fuji may be explained in the same way. At several points on the east slope permafrost was not found, possibly because the depth to the permafrost table there is comparatively deep. It may be possible to explain this difference by a variation in geothermal conditions, since the existence of the weak fumaroles was reported in this area. But a detailed study of this problem will be necessary.

The mean value of the depth to the permafrost table at the ten observation points at the crater rim, omitting the two points on the east slope, is 64.3 cm, the mean value of their altitude being 3,732 m. The depth to the permafrost table at places lower than the summit was observed along a ridge extending north-east from the crater rim, as shown in the upper part of Fig. 1. The permafrost table was, however, too deep to measure by the simple sounding method at places lower than 3,500 m. Retzer³ described the areas in which there is permafrost in the high mountains of the west of the United States of America. These areas are on mountain ridges generally above 3,200 m in mid-latitudes further north than 36° N . Therefore, the lower limit of permafrost at Mount Fuji could be expected to be nearly the same.

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Submillimetre Wave Absorption Anomalies in the Lower Atmosphere and the Existence of Water Dimers

THERE is growing evidence^{1–6} that the presence of a finite concentration of vapour phase dimers of water has to be considered if atmospheric absorption in the submillimetre region is to be satisfactorily explained. So far all the observations of the open atmosphere in this wavelength region, where there are features attributable to dimers, have been made with radiation from the Sun coming through the whole atmosphere in a slant path.

If the concentration of dimers in the atmosphere reaches equilibrium, it should vary as the square of the water vapour pressure, and because water vapour is normally concentrated low in the atmosphere, it is to be expected that dimer absorption will be predominantly a low altitude phenomenon. It is therefore of considerable interest to make observations over a horizontal path in the open atmosphere to determine whether the same features are present that we find in slant path observations. The conditions in which these spectral features are seen are restricted because they overlap with much stronger features of the water monomer. We have therefore closely

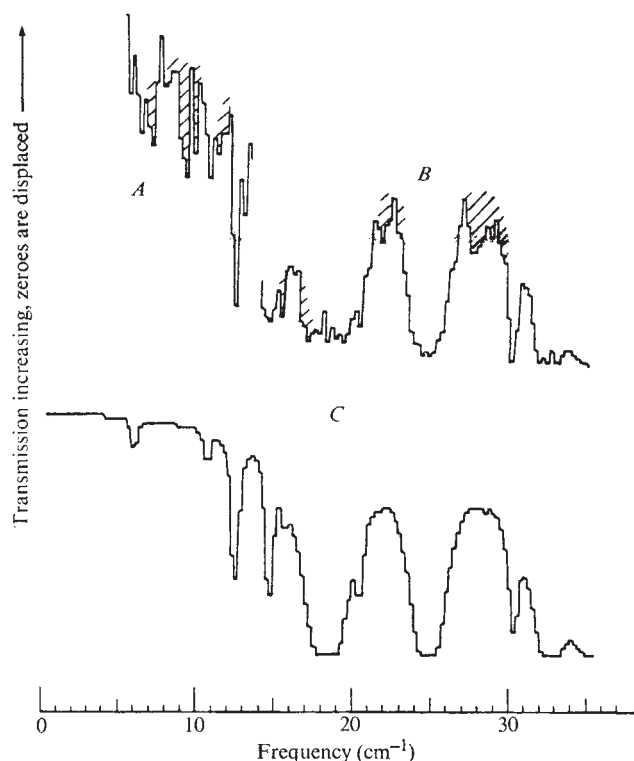


Fig. 1 Absorption by atmospheric water vapour in a 150 m horizontal path. *A* and *B*, Horizontal path mean conditions: *A*, 38 cm atm, pressure 1.9 mm Hg, temperature 270 K; *B*, 50 cm atm, pressure 2.5 mm Hg, temperature 277 K. *C*, Theoretical value from calculated water monomer lines with Zhevakin-Naumov line shapes. 50 cm atm, over-pressure 1 atm, temperature 277 K. Resolution is 0.5 cm⁻¹.

followed the conditions in which they were seen in the slant path experiment. The total amounts of water have been chosen to be comparable in the two kinds of experiment.

Absorption spectra of the atmosphere were recorded in the range 5.5 cm⁻¹ to 35 cm⁻¹ in an open path, 150 m long, on the grounds of the National Bureau of Standards in Boulder, Colorado (altitude 5,500 feet). The source was a mercury vapour lamp at the focus of a 2 m diameter searchlight mirror. The radiation was chopped and a phase-locking signal was transmitted to the detector amplifier. The receiver used a sixteen inch Cassegrain telescope with an NPL modular interferometer and Golay cell detector. We obtained spectra at a resolution of 0.5 cm⁻¹ using standard Fourier methods.

Observations were made during several weeks, from November 1970 to February 1971. We did not seek to correlate variations of the individual spectra with meteorological conditions, but did treat the observations in two groups, to provide two average values of the amount of precipitable water in the path, and for two spectral pass bands. The results are shown in Fig. 1 as relative transmission spectra. The additional short path observations were made using the same mercury lamp as was used in the long path. We have not attempted to obtain quantitative values for the transmission because there are some undetermined geometrical factors, but these do not affect the spectral distribution.

Fig. 1 shows the new spectra with a theoretical contour of monomeric water vapour absorption added for comparison. Fig. 1*A* gives the results from 5.5 cm⁻¹ to 13 cm⁻¹ for a mean amount of water vapour of 38 cm atm (equivalent to 0.35 mm precipitable water) at 270 K. Fig. 1*B* gives results from 13 cm⁻¹ to 35 cm⁻¹ for a mean amount of water of 50 cm atm (equivalent to 0.4 mm precipitable water). The theoretical contour in Fig. 1*C* is calculated for the whole spectral range and for the higher amount of water vapour and temperature. We have used a tabulation of water monomer line positions,

strengths and widths which was made available to us by R. F. Calfee and derived from original work of W. S. Benedict. The Zhevakin-Naumov model was chosen for our calculations, but the differences between the predictions of this model and the older Van Vleck-Weisskopf model are very much less than between either and the observations. We have not included magnetic dipole pure rotation oxygen lines, because these will have a very small contribution over our 150 m path. There are major differences between observation and prediction, which we have shown shaded in the figure, particularly in the regions 7–11 cm⁻¹, 17 cm⁻¹, 22 cm⁻¹ and 28 cm⁻¹. These parallel very closely results obtained from slant path work and which, at least in part, have been attributed to the absorption by dimeric water vapour.

Rough quantitative values of absorption by the new features may be derived from intercomparison with known monomer features, such as that at 12.5 cm⁻¹, but it should be kept in mind that dimer absorption may also be present there, overlaid by the strong monomer line.

Two warnings are essential before interpreting these results. We have no evidence that the absorption features we observe are necessarily due to a species in equilibrium in the atmosphere. Many of the present observations were made over snow (as were the slant path observations at Jungfraujoch, Switzerland) and it should be kept in mind that evaporation from snow might be a contributing factor. Secondly, though evidence has been given³ in support of the water dimer assignment, the possibility that other molecular complexes may be present in the atmosphere should not be overlooked. Further laboratory researches will be necessary to make definite assignments.

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BIOLOGICAL SCIENCES

Visual Sensations produced by Single Fast Particles

It has long been known that the sensation of light (phosphene) can be produced by the effect of ionizing X-radiation on the retina¹⁻³. The threshold dose for a phosphene falls with dark adaptation but always corresponds to the simultaneous arrival of many X-ray photons. Little attention has, however, been paid to the question of whether single fast particles can produce phosphenes, with the exception of the pioneering attempts by D'Arcy and Porter⁴ to detect single relativistic cosmic-ray muons.

This has become of topical interest because of the reports of Apollo astronauts that they saw "flashes of light" while in translunar flight⁵; the astronauts observed these flashes most easily when they were dark-adapted. It has been suggested⁶ that the flashes were caused by single cosmic-ray nuclei which

penetrated the cabin walls and entered the eye to cause the sensation of light, either through the mechanism of optical Cherenkov radiation within the retina of the eye or by some form of direct excitation. We have conducted experiments with 3 MeV and 14 MeV neutrons to investigate the possibility of direct excitation effects—any secondary charged particles generated within the eye were well below the Cherenkov threshold (~ 500 MeV for protons in the vitreous humour). During the preparation of these experiments, we received reports that Fremlin⁷ had observed “points of light” while partially dark-adapted in a $10^5 \text{ cm}^{-2} \text{ s}^{-1}$ flux of neutrons of mean energy 3 MeV and maximum energy 8 MeV, and that Tobias and his coworkers⁸ had also seen point-like flashes when dark-adapted in a $2 \times 10^4 \text{ cm}^{-2} \text{ s}^{-1}$ flux of neutrons of energy 20 to 640 MeV.

The monoenergetic 3 and 14 MeV neutrons used in our experiments were produced by the action of a beam of deuterons from a Cockcroft-Walton accelerator on deuterium and tritium targets respectively. A small blockhouse consisting of successive layers of concrete, lead and boron-loaded paraffin wax shielded the observer from the essentially isotropic spray of neutrons emanating from the target; the neutron beam entered the blockhouse through a hole of diameter 5 cm. The target-head distance was 45 cm so the collimation of the beam from the 1 cm diameter target was comparatively crude. The beam fluxes and radiation dose rates were carefully measured before exposure of any observer, and monitoring continued throughout the experiment.

Four observers viewed the neutron beam in several beam-head orientations; each observer received a dose equivalent of about 100 mrem to the head and about 20 mrem or less to the body. The blockhouse design excluded all external light and each observer was dark-adapted for a minimum of 30 min before exposure. All observers were aware of the effects reported by previous observers.

Two observers separately viewed a flux of $2 \times 10^4 \text{ cm}^{-2} \text{ s}^{-1}$ of 3 MeV neutrons. No distinguishable visual effects were reported in the course of either a 1 min frontal exposure of one eye or a similar exposure with the beam entering the back of the head. When the experiment was repeated with a frontal exposure to a $2 \times 10^4 \text{ cm}^{-2} \text{ s}^{-1}$ flux of 14 MeV neutrons, the first observer immediately reported flashes. Similar phenomena were reported by the other observers and the results obtained for different head-beam geometries, normally using 10 s beam exposures, can be summarized as follows. (i) For a beam entering one eye frontally (visual axis parallel to the beam), all observers reported seeing “streaks” or “lines” of differing length up to a maximum corresponding to a visual angle of $\sim 2^\circ$. Most streaks apparently occurred at visual field angles ~ 30 to 60° and were approximately radial with respect to the visual axis. The edges of the streaks appeared sharp, although one observer described the ends as “fuzzy”. The streaks were bluish-white or colourless and were uniform in brightness along their length; they occurred at rates varying from $\sim 0.3 \text{ s}^{-1}$ to $\sim 10 \text{ s}^{-1}$ for different observers. At the centre of the field fainter, but more numerous, “points” or very short lines were seen by three of the observers. All these phenomena were of short duration. (ii) For a beam entering both eyes laterally (visual axis perpendicular to the beam), all observers saw a higher rate of streaks. The streaks again had a range of lengths up to a maximum corresponding to a visual angle $\sim 2^\circ$ and were predominantly horizontal (that is, parallel to the beam direction) with deviations generally $< 30^\circ$. They appeared at most places in the field of view but were more numerous near the periphery. Points were less noticeable with this geometry, possibly because of the greater number of streaks. (iii) For a beam entering the rear of the head (visual axis parallel to the beam), only one observer saw flashes, at a rate $\sim 0.3 \text{ s}^{-1}$. The observers were normally told when the beam was turned on or off, but each observer had one session of types (i) or (ii) when he was warned that during the following minute, the beam would be turned on for a short

interval. All observers correctly identified the time at which the beam was turned on or off.

The particle energies used in our experiment, unlike those of Tobias⁸, rule out the possibility that these visual phenomena were a consequence of optical Cherenkov radiation in the eye fluids and tissues; this explanation may, of course, still be valid for the flashes seen by the astronauts. The appearance of the streaks agrees well with that expected from particles moving approximately in the direction of the beam and intercepting and somehow stimulating a retinal receptor layer $\sim 60 \mu\text{m}$ thick on the surface of an eyeball of diameter $\sim 2 \text{ cm}$. According to this interpretation, the maximum possible intercept length with the receptor layer for a particle travelling tangentially is $\sim 1 \text{ mm}$, equivalent to a visual angle $\sim 3^\circ$; this corresponds fairly well with the observers’ necessarily crude estimates of the longest streaks seen.

The observation of such long streaks also sets a lower limit on the range of the particles responsible. The great reduction in the number of flashes observed when the beam entered the back of the head (an effect also reported by Fremlin⁷ and due presumably to attenuation of the neutron beam by the skull and brain tissue before it reaches the retina) suggests that it is very unlikely that the flashes are produced by direct stimulation of the visual cortex. It is also improbable that direct stimulation of the optic nerve could adequately account for the directional and other properties of the flashes.

It seems most likely that the flashes are caused by the direct stimulation of either the retinal receptors, probably the rods, or their associated nerve cells by particles with ranges $\gtrsim 1 \text{ mm}$. During a frontal exposure of a single eye to the flux of 14 MeV neutrons, we would expect 20 knock-on protons s^{-1} of energy $> 8 \text{ MeV}$ and range $> 1 \text{ mm}$ to interact with the retinal layers to produce streaks; this agrees reasonably well with the observed flash-rate. It seems unlikely that the longer streaks, at least, could be caused by heavy recoil nuclei resulting from neutron collisions with the C, N and O atoms of the tissue because these would have insufficient range. Other processes, such as the $^{16}\text{O}(n,\alpha)^{13}\text{C}$ reaction, may contribute to the more point-like flashes, but their cross-sections are generally small. The original neutrons themselves cannot, of course, be directly responsible for the flashes, because their range suggests a flash rate far in excess of that observed.

It is significant that the streaks appeared constant in brightness along their length to all observers, in spite of considerable variations in the specific ionization of any charged particle in the relevant energy region along track lengths $\sim 1 \text{ mm}$. The sensation of brightness cannot therefore be proportional to the ionization produced in each retinal receptor; the receptor is probably rather simply “turned on” by the passage of the particle. The comparative ease with which the streaks were seen as compared with the points or short lines may be due partly to areal integration effects and partly to the greater ease with which the brain can distinguish elongated features from the background noise of the eye as experienced by a dark-adapted observer. This may also explain the absence of effects with the 3 MeV beam—the range of a 3 MeV knock-on proton is only $\sim 150 \mu\text{m}$ so the maximum possible streak length only corresponds to $\sim 0.5^\circ$. Fremlin’s positive result⁷ at similar neutron energies is probably accounted for by the high flux of his beam (a factor ~ 5 greater than ours) and its content of neutrons up to 8 MeV in energy.

As all our effects were close to threshold and their brightness did not apparently depend on the specific ionization, it is difficult to see how similar direct excitation effects could account for the observations of the astronauts, which were almost certainly made in far less favourable conditions of dark adaptation. If, however, the excitation could somehow be extended laterally from the particle track, more receptors might be stimulated and hence a greater sensation of brightness produced. We speculate that such a role might be played by energetic δ -rays (with ranges $\sim 100 \mu\text{m}$) produced by relativistic cosmic-ray nuclei of high atomic number passing through

the eye. It has been made clear in the discussions of the Cherenkov mechanism⁶ that the flux of cosmic-ray nuclei with $Z \geq 6$ is compatible with the flash rates reported by the astronauts. We therefore consider that it remains possible that either or both mechanisms are responsible for these flashes. The use of single, high energy particles may eventually prove a useful additional probe for exploring the properties of the retinal receptors.

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Musical Tests for Functional Lateralization with Intracarotid Amobarbital

WHEN brain surgery which may affect language mechanisms is being contemplated, lateralization of speech is sometimes ascertained before operation by injecting 200–250 mg of sodium amobarbital ('Amytal') into either right or left common carotid artery, which depresses for several minutes the corresponding cerebral hemisphere¹; transient contralateral hemiparalysis and temporary loss of the higher functions dependent on the depressed hemisphere are produced. Although usually used when speech dominance is questionable^{2–4}, amobarbital testing is also indicated in persons with apparently well established laterality when they are to undergo commissural section as a treatment for seizure disorder⁵. Even where definite right-handedness clearly implies left hemisphere speech dominance, right carotid amobarbital testing can ensure that the preservation of speech does not require right hemisphere participation. This affords an independent approach to the further exploration of the higher functions, for which the right hemisphere may be dominant⁶. We report here some of the results of right carotid injection in six patients (of P. J. Vogel) who had no previous operative intervention, and had a consistent right-handed preference for a wide variety of tasks including writing, eating and tool use.

Right temporal lobectomy causes deficiencies in timbre and tonal memory not resulting from left lobectomy⁷; singing is possible after the removal of the entire left ("major" or dominant for speech) hemisphere⁸, and work done so far indicates musical deficiencies from right hemisphere strokes not affecting speech⁹. Therefore we decided to look for musical deficiencies during "minor" hemisphere depression.

After injection the short time span of maximum depression limited the examination to gross changes in musical ability. Accordingly, the patients were asked to sing some familiar songs (such as "London Bridge" or "Happy Birthday") before, during, and after hemisphere depression, avoiding words by substituting "la, la, la . . ." in most cases. The singing was interspersed with other tasks whose principal purpose was to establish that the patient could follow commands with the unparalysed right limbs, recite words, answer questions, name objects and recall previous events with only the left hemisphere functioning. The entire procedure was rehearsed and tape recorded after insertion of the carotid needle but before amobarbital injection. After injection the examiner frequently requested movements of the paralysed side of the body (for example, "make a fist", "stick out your thumb", and so on); when these commands could be followed (usually 3 to 5 min after injection) further examples of speaking and singing were recorded and the testing was terminated.

As well as producing the expected paralysis of the left side of the body, right carotid injection grossly disturbed singing in all six patients. In five of the six cases, speech remained unaffected except for slight slowing and slurring of words and the presence of some monotonicity; the intelligibility and rhythmicity of speech were hardly affected. One patient would neither sing nor talk until 8 min after injection. Two patients talked but would not sing until 3 min after injection, when singing was moderately impaired. In the remaining three patients, singing was markedly impaired during the first 2 min and moderately impaired throughout the rest of the session. The typical defects were monotonicity with a few unnatural pitch changes. The patients had difficulty in spontaneously beginning a song on request but succeeded after the examiner had sung the first few bars. It was apparent, however, that the patients were monitoring their own singing as indicated by unsuccessful attempts to change pitch at appropriate times in the song. Also, correct rhythm without melody indicated recognition of the song. In one case, words correctly replaced "la, la, la . . .", but the tune was amelodic.

The retention in five patients of appropriate conversation in spite of complete paralysis of the left limbs demonstrated the ability of the left hemisphere alone to subserve verbal functions while unable to sustain singing. Conversely, the melodic deficiency when speech was only minimally disturbed indicated that the amobarbital-depressed right hemisphere in these right-handers was critical for certain musical functions. These findings are consistent with previous reports^{7,10–12}, suggesting a dominance of the "minor" hemisphere in tonal abilities. Alternatively, the results can be interpreted assuming that singing is a bilateral function, requiring the cooperation of both hemispheres. The resolution of this ambiguity will require similar testing during separate anaesthetization of the left hemisphere. Left carotid amobarbital injection was medically indicated for only two of our subjects. In one such case, there was no vocalization for 2 min and then melodic (although slurred) singing occurred just before the return of the ability to repeat single words. The other patient was silent for 7 min, after which she spoke and sang equally well. The left hemisphere contribution remains uncertain, but it seems that the right hemisphere is more important for singing than speech.

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Fourier Analysis of Evoked Potentials and Human Intelligence

PARAMETERS of the Fourier transform of averaged evoked potentials (AEP) have recently been related to psychometric intelligence^{1,2} and I have attempted a similar analysis of a larger sample of subjects of high and low intelligence. This was carried out several months before the publication of the previous reports. Because the original objective was not replication, there are inevitably some major differences of technique. In general, this is a field in which meaningful replication is difficult³, but there are grounds for believing that the differences between this and the previous techniques and recording locations probably do not explain the different results.

From an FM tape library of more than 1,000 subjects I selected 164 who met the following criteria. The first was that they had IQ scores of over 120 on two out of three of the Otis, PMA and WISC IQ tests. This yielded ninety-three subjects classified as high IQ subjects. The second criterion was that they had IQ scores of less than 85 on two out of the three IQ tests. This yielded seventy-one low IQ subjects. The total sample represents all subjects available to me who met the criteria. The mean age of the high IQ subjects was 11 yr with a range of 7.5 to 14.6 yr. The mean age of the low IQ subjects was 11.6 yr, with a range of 7.5 to 15.3 yr. Of the total, ninety-two were males and seventy-two were females.

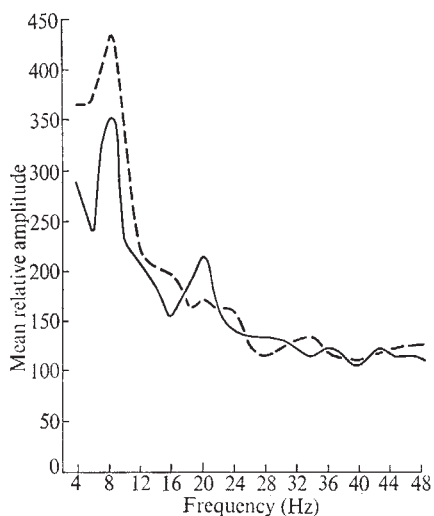


Fig. 1 Fourier analysis of the average evoked potential 4.5 s after stimulation with a 2 Hz resolution. Fourier amplitudes of ninety-three high IQ (—) and seventy-one low IQ (---) subjects are compared.

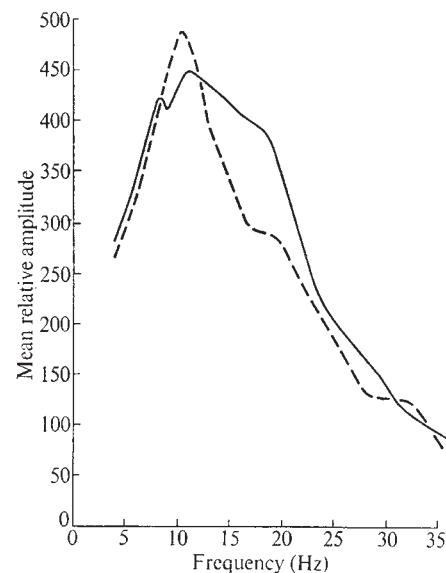


Fig. 2 Fourier analysis of the average evoked potential 4.25 s after stimulation with a 1 Hz resolution. Fourier amplitudes of ninety-three high IQ (—) and seventy-one low IQ (---) subjects are compared.

Data on FM tapes were as follow. The EEG was derived from bipolar scalp contact electrodes 6 cm apart astride C₄ in the 10–20 system. The EEG was recorded in a band width 3 dB down at 3–50 Hz with an attenuation slope of 24 dB per octave. Trigger pulses corresponding to the onset of photic stimuli were also recorded. These analog data were digitized at a rate of 500 samples/s⁻¹ and six bit amplitude resolution. The first 256 samples after the stimulus (representing 512 ms of data) were recorded on an IBM compatible seven track digital magnetic tape. Fourier analysis was then performed on an IBM 360–65 system in two ways: (1) the two first 500 ms giving a resolution of two cycles, and (2) the first 250 ms but with a 1 cycle resolution⁴.

There are no significant differences between the average Fourier transforms of a group of high and low IQ subjects (Fig. 1). In the 12–14 Hz region where Weinberg² found maximal correlation with the density of spectral components (that is, the square of the Fourier amplitudes), I found no significant differences in the Fourier transform over 0.5 s (Fig. 1) or over 0.25 s (Fig. 2).

There is some indication⁴ that high IQ subjects tend to have higher frequency components in the first 200 ms after stimulation. Between 200–500 ms, the frequency of the components is about the same for both high and low IQ subjects. I checked this by performing a Fourier analysis on the same subjects over 250 ms (Fig. 2) at 1 Hz resolution. There is no significant difference ($P > 0.05$) between the curves with respect to IQ. There is, however, considerably more energy at frequencies above 12 Hz with both high and low IQ subjects in the first 250 ms (Fig. 2) than in the first 500 ms (Fig. 1).

The “natural frequency of the dominant function” as defined by Freeman⁷ was not measured; but it is probably approximately the same as the frequency of maximum amplitude in the Fourier transform. This frequency is obviously (Figs. 1 and 2) in the alpha range (8–12 Hz) for both high and low IQ subjects and is therefore uncorrelated with intelligence. Weinberg² and Bennett¹ used occipital derivations. I have compared the Fourier transform of the AEP from occipital and sensory motor areas in these three subjects and found them to be essentially similar with a large peak in the alpha band. Furthermore, the principal features of the AEP from different cortical locations look very similar⁸. It is therefore unlikely that electrode placement would account for the differences in results. The Fourier transform of the AEP is a complete mathematical description of the AEP of a

particular subject: thus I am unable to confirm the findings of Bennett and Weinberg.

In looking for electrophysiological correlates of intelligence, it may be better to ignore the spectral characteristics of the AEP³. Components of the AEP correspond to neural events in the processing of information in the brain; the latency of these components is very stable but their amplitude and spectral characteristics are not. Any analysis which depends on average characteristics of the AEP over an interval of time may be hard to relate to human intelligence. Analyses which are based on ratios, relative component amplitudes, peak latencies, the first and second derivatives of the AEP, and so forth, seem more promising. Large scale studies using discriminant analysis⁹ may also reveal critical differences in the output of high and low IQ brains.

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Identification of Prostaglandin F_{2α} released by Distension of Guinea-pig Uterus *in vitro*

DISTENSION of the guinea-pig uterus early in the oestrous cycle significantly shortens the cycle¹, as the size of the corpora lutea shows. Furthermore, distension of one uterine horn affects only the corpora lutea of the ipsilateral ovary². Thus there seems to be a local utero-ovarian relationship. Total hysterectomy³, severance of the vascular tissue between the ovary and uterus^{4,5}, or destruction of the endometrium⁶ significantly lengthens the oestrous cycle. Hemi-hysterectomy affects only the ipsilateral corpora lutea⁷. Transplantation of the uterus or of the endometrium alone maintains slightly extended oestrous cycles⁸. A nervous mechanism cannot account for all these observations, and it has been suggested that a luteolytic substance is released from the endometrium and reaches the ovary, possibly through the blood or lymphatic vessels, causing the corpora lutea to degenerate.

Prostaglandin F_{2α} which has been isolated from human endometrial tissue⁹ has a luteolytic action in various species^{10,11} including the guinea-pig¹². It therefore seemed reasonable to propose that release of a prostaglandin might account for the luteolytic effects we have described. We have measured the output of prostaglandin from the uterus in one condition which is associated with premature luteolysis, uterine distension.

In the first experiment a female guinea-pig weighing about 500 g was smeared daily by a lavage technique and killed on day 3 of the oestrous cycle (day 1 is the day before the post-ovulatory influx of leukocytes). The two uterine horns were removed and separated. One horn was distended by intraluminal insertion of a piece of polyethylene tubing (3 mm wide and 30 mm long). The other horn was treated similarly, except that the polyethylene tubing was removed immediately. Both horns were incubated separately at 37° C in 7 ml. of Tyrode

solution for 3 h and gassed with 5% CO₂ in oxygen. The two incubated solutions were assayed on the rat fundal strip in terms of prostaglandin E₁ (PGE₁) which is 1.5 times as potent as prostaglandin F_{2α} on this preparation (Table 3).

The sample from the distended horn contained 65 ng of PGE₁ equivalent/ml. The sample from the undistended horn contained no detectable activity (<40 ng of PGE₁/ml.).

The use of the appropriate antagonists showed that the activity in the test sample was not due to 5-hydroxytryptamine, histamine or acetylcholine. Nor were any of the smooth muscle contracting peptides present, for activity remained after incubation with chymotrypsin.

In the second experiment nine female guinea-pigs, weighing about 500 g, were killed on day 3 or 4 of the oestrous cycle. One horn from each animal was distended as before (test), the other horn from each being untouched (control). Each horn was incubated in 8 ml. of Tyrode solution in the same conditions as in the first experiment. In eight out of nine experiments the amount of smooth muscle contracting activity in the Tyrode bathing the distended horn exceeded that from the non-distended horn (Table 1).

The individual test samples and control samples were then pooled separately to give one test and one control sample. These were treated in parallel by the solvent extraction procedure for the isolation of prostaglandin-like material¹³, omitting the initial ethanol state.

The two final dry extracts (test and control) were dissolved in 2 ml. of water and assayed on the rat fundal strip. The test

Table 1 Assay of the Nine Pairs of Incubated Samples obtained in the Second Experiment on the Rat Fundal Strip

Sample	Activity = ng of PGE ₁ /ml.	
	Test (distended horn)	Control (non-distended horn)
1	12	7.5
2	10	2.5
3	15	n.d. (<3)
4	10	10
5	14	n.d. (<3)
6	16	n.d. (<3)
7	18	7
8	14	7
9	20	n.d. (<3)

n.d., No detectable activity.

Table 2 Assay of the Fractions obtained from the Silicic Acid Columns in the Third Experiment

Fraction number	Eluant: ethyl acetate in benzene	Assay on rat fundus	
		Test	Control
0	Original flask	80 ng E ₁	<25 ng E ₁
1	40 ml. 30%	<25 ng E ₁	<25 ng E ₁
2	40 ml. 40%	<25 ng E ₁	<25 ng E ₁
3	20 ml. 60%	<25 ng E ₁	<25 ng E ₁
4	110 ml. 60%	30 ng E ₁	25 ng E ₁
5	20 ml. 60%	<25 ng E ₁	<25 ng E ₁
6	150 ml. 80%	1,000 ng F _{2α}	100 ng F _{2α}
7	40 ml. 100%	<50 ng F _{2α}	<50 ng F _{2α}
8	50 ml. Methanol	100 ng F _{2α}	70 ng F _{2α}

Table 3 Results of the Parallel Assay of the 80% Fraction obtained from the Silicic Acid Columns in the Experiment assayed against PGE₁ and PGF_{2α} on Four Preparations

Assay preparation	Activity in test sample		Activity in control sample	
	PGF _{2α}	PGE ₁	PGF _{2α}	PGE ₁
Rat fundus	1 µg	1.5 µg	100 ng	150 ng
Rabbit jejunum	1 µg	10 µg	63 ng	630 ng
	-2 µg	-20 µg	-125 ng	-1.25 µg
Jird colon	1.2 µg	600 ng	87.5 ng	43.7 ng
	-1.4 µg	-700 ng	-100 ng	-50 ng
Guinea-pig ileum	1.3 µg	67 ng	Insufficient to assay	
	-2 µg	-100 ng		

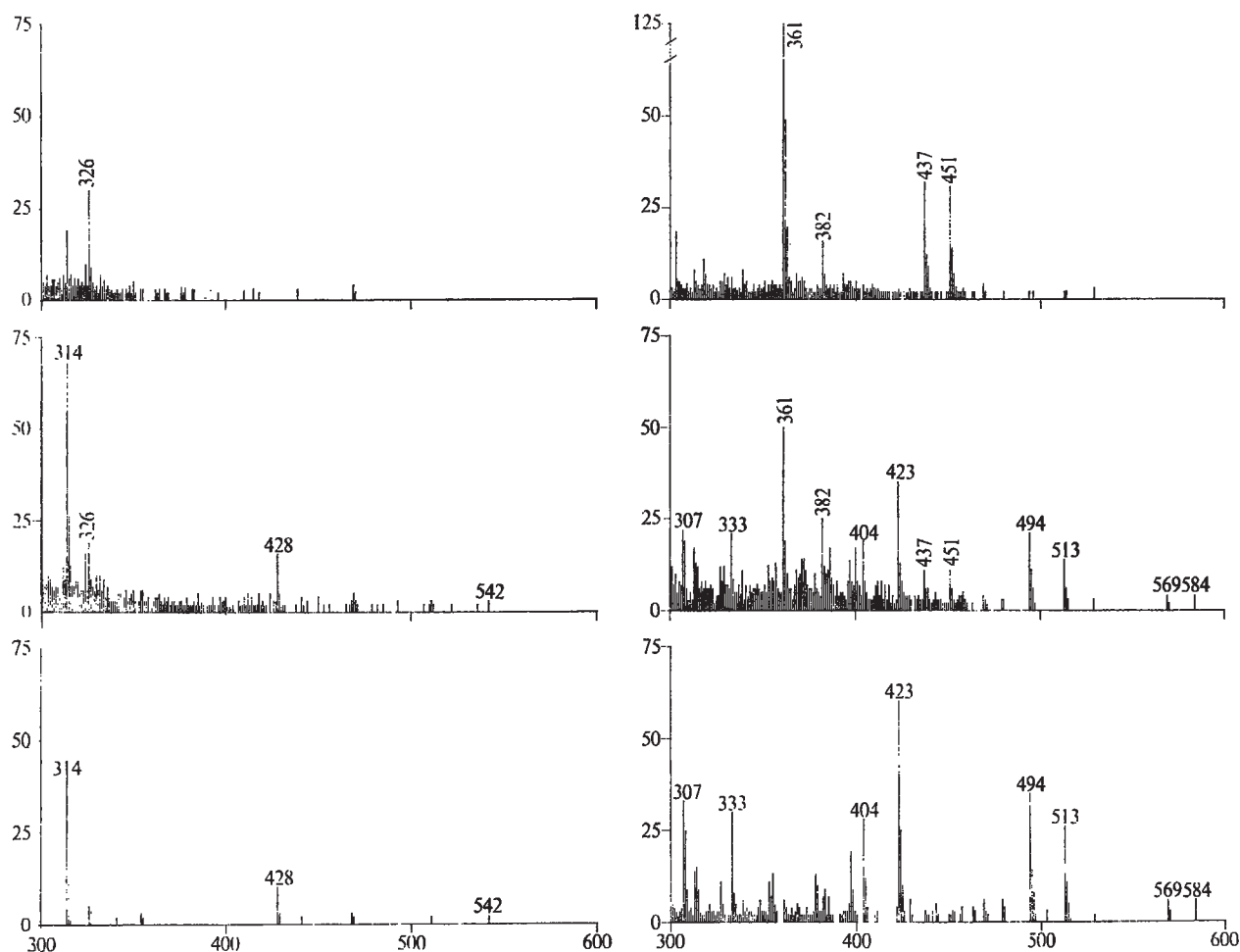


Fig. 1 Line diagrams of mass spectra (m/e peaks greater than 300) of Me/TFA (left) and Me/TMS (right) derivatives of control (upper), test (middle) and authentic $F_{2\alpha}$ (lower) samples. Principal peaks characteristic of $F_{2\alpha}$ are indicated by horizontal numbering; other major peaks are indicated by vertical numbering.

sample contained 40 ng of PGE_1 equivalent, whereas the control sample contained no detectable activity (<10 ng of PGE_1). The active principle in the test sample thus behaved like a prostaglandin in its partition and one pharmacological property.

In a further control experiment with one pair of uterine horns, one horn was incubated in the presence of a piece of polyethylene tubing, though not distended by it; the other horn was incubated alone. There was no difference in prostaglandin concentration ($\equiv 5$ ng of PGE_1 /ml.) of the incubation media of these two horns, indicating that the presence of polyethylene tubing did not affect output. Two pieces of polyethylene tubing were incubated in the absence of uterine tissue but no smooth muscle stimulating activity could be detected in the incubation media.

The difference in biological activity shown in Table 1 was therefore not attributable to leaching of the polyethylene tubing nor to an interaction between the polyethylene tubing and uterine tissue.

In the third experiment we attempted to identify more conclusively the substance released when the uterus distends. Thirty-five guinea-pigs were killed on day three of the oestrous cycle and the uterine horns dissected out and separated. One horn from each was distended as in experiment 1 (test), the other horn serving as the control. Each horn was incubated in 10 ml. of Tyrode and gassed with oxygen at $37^\circ C$ for 5 h. The incubated samples obtained were pooled to give 250 ml. of test sample and 245 ml. of control. Both samples were extracted by solvent partition as in the second experiment and then subjected to silicic acid column chromatography¹⁴. The fractions obtained from the column were evaporated to dryness *in*

vacuo, the residue being dissolved in 1 ml. of water and assayed on the rat fundal strip. The results are shown in Table 2.

Using authentic prostaglandins, columns run with this batch of silicic acid showed that PGEs were eluted by 60% ethyl acetate in benzene while PGFs were eluted in the 80% fraction. Table 2 shows that the amount of PGE-like material in the test and control samples was small and nearly identical. But there was ten times as much PGF ($\equiv 1 \mu g$ $PGF_{2\alpha}$) in the test sample as in the control.

This prostaglandin F fraction was assayed on three further biological preparations: the ascending colon of the Mongolian jird (*Meriones unguiculatus*), the rabbit jejunum and the guinea-pig ileum (Table 3). This parallel assay showed that for the prostaglandin F fraction of the test sample (distended horn) the estimated amount of activity was similar on four tissues when assayed in terms of $PGF_{2\alpha}$ (greatest index of discrimination = 2). The estimates differed widely when the sample was assayed in terms of PGE_1 (greatest index of discrimination = 297). Further, the responses of the rabbit jejunum and guinea-pig ileum to PGE_1 and $PGF_{2\alpha}$ differed qualitatively, the responses to the test sample resembling those produced by $PGF_{2\alpha}$. Tentative identification of this substance as $PGF_{2\alpha}$ was obtained by combined gas chromatography-mass spectrometry. The substance released from the non-distended horn also resembles $PGF_{2\alpha}$ but the amount was ten to twelve times less than that from the distended horn.

The previous experiment was repeated using thirty guinea-pigs. The fractions from the silicic acid columns were assayed solely on the rat fundal strip, producing similar results as in the third experiment.

There was again $PGF_{2\alpha}$ -like material (650–700 ng) in the

test sample (distended horn), this time eight times as much as in the control. Fraction 6 (the "PGF fraction") of both test and control samples was analysed by gas chromatography and mass spectrometry¹⁵. The methyl ester/tri-methyl silyl ether (Me-TMS) and methyl ester/trifluoroacetate (Me/TFA) derivatives of the test samples gave gas chromatographic peaks at 19 and 6 min respectively, corresponding to the retention times of the same derivatives of authentic PGF_{2α}. The amount of PGF_{2α} in the test sample estimated by this means agreed with the result of biological assay. The control sample gave no detectable peaks at these times.

Mass spectra of the Me/TMS and Me/TFA derivatives of control, test and authentic PGF_{2α} samples were recorded at the appropriate retention time. Line diagrams of *m/e* peaks greater than 300 are shown in Fig. 1. The Me/TMS derivatives of the control and test samples have prominent peaks in common at *m/e* 361, 382, 437 and 451. In addition the test, but not the control, has prominent peaks at *m/e* 307, 333, 404, 423, 494, 513, 569 and 584. These eight peaks are characteristic of authentic PGF_{2α}. Similarly the Me/TFA derivatives of control and test have one common peak at *m/e* 326, but the test has three additional peaks at *m/e* 314, 426 and 542 corresponding to the three major peaks of the Me/TFA derivative of PGF_{2α}. It is concluded from this evidence that the test sample contains PGF_{2α}.

On the basis of gas chromatography neither PGF_{1α} nor 5α, 7α-dihydroxy-11-oxo-tetranorprostanic acid (the principal urinary metabolite of PGF_{2α} in the guinea-pig) could be detected in the test or control samples.

The evidence from solvent extraction, chromatographic behaviour, quantitative parallel biological assay, and particularly gas chromatography and mass spectrometry prove that the biologically active substance released by distension of the guinea-pig uterus *in vitro* is prostaglandin F_{2α}.

Distension of the guinea-pig uterus *in vivo* results in premature luteolysis¹. We have now shown that distension of the guinea-pig uterus *in vitro* releases prostaglandin F_{2α}, a substance known to possess luteolytic activity in guinea-pigs¹². The amounts of prostaglandin F_{2α} used by Blatchley and Donovan¹², however, were far in excess of the amounts we have found to be released by the uterus. It is probable that much smaller doses of prostaglandin F_{2α} will produce a luteolytic effect when injected into guinea-pigs, though the route of administration would have to be chosen carefully because 95–99% of the prostaglandin F_{2α} in the blood is removed by one passage through the lungs and liver¹⁶. But the fact that prostaglandin F_{2α} possesses luteolytic activity and that we have been able to demonstrate its release from the guinea-pig uterus by distension provides some evidence for the hypothesis that luteolysis itself is prostaglandin F_{2α}.

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Luteolysis and Ovulation induced by Prostaglandin F_{2α} in the Hamster

F_{2α} is one of the several naturally occurring prostaglandins (PGs). It is present in relatively high concentration in the human uterus, and in menstrual and amniotic fluid^{1,2}. Its precise role in reproductive endocrinology is the subject of intense research. F_{2α} exerts antifertility effects in several species including the hamster³. The endocrine basis for this remains obscure, although decreased secretion of progesterone after a single injection of F_{2α} into hamsters has been observed⁶. In this report we provide further evidence that this antifertility effect is brought about by the luteolytic property of the PG and is accompanied by ovulation.

Adult hamsters weighing approximately 100 g were obtained commercially and those with regular, 4-day oestrous cycles were used for experiments. The animals were housed in controlled light conditions (from 0600 to 2000 h) and temperature (72 F°) and given a standard diet. They were caged with fertile males on the day before expected external vaginal discharge and the next morning vaginal smears were taken. If clumps of spermatozoa were found in the smear, the day was designated day 1 of pregnancy. Some hamsters were bilaterally hysterectomized by thermocautery on day 4 of pregnancy. The uterus was resected at the utero-tubal junction leaving the Fallopian tubes intact and at the posterior end at its cervical junction. The racemic mixture of F_{2α} was prepared in phosphate buffer (pH 7.4) and injected subcutaneously, once daily, in a volume of 0.25 ml. from days 4–6 inclusive of pregnancy. All the treated and appropriate control hamsters were killed on day 8 and the number of implantation sites counted. The ovaries were carefully examined and the number and condition of corpora lutea noted. They were fixed in formal saline and serially sectioned for histological examination. Both Fallopian tubes were examined for the presence of tubal ova which were counted.

In the doses used (50 to 100 µg/day), F_{2α} had no detectable toxic effect and all treated hamsters gained in weight comparably with controls. No embryos were found at autopsy in hamsters injected with F_{2α} (group B). Examination of ovaries showed two sets of corpora which seemed to be of different ages. Histologically, one set appeared to be degenerating while the other set appeared to be freshly formed. The ovaries of control hamsters had only one set of corpora lutea which was accompanied by healthy implantation sites (group A). The follicles in the ovaries of both control and treated animals appeared to be healthy. When the Fallopian tubes were flushed, four out of six treated hamsters had a mean of 3.6 ± 1.5 ova while the controls had none. This suggests that the treated hamsters had ovulated recently, which would account for the two sets of corpora in the ovaries. Thus the anti-fertility effect of the PG may have been due to degeneration of corpora lutea with a resultant deficiency of progesterone. This hypothesis was supported by the finding that concurrent administration of the steroid (4 mg/day, subcutaneously in

Table 1 Effects of Prostaglandin $F_{2\alpha}$ on Implantation Sites, Corpora Lutea and Ovulation in Hamsters

Group and treatment	Dose/day sc	No. hamsters pregnant No. treated	No. hamsters with old corpora lutea (mean + s.e.)	No. hamsters with new corpora lutea	No. hamsters with tubal ova (mean + s.e.)	Body-weight change (%)
A Pregnant controls	0	7/7	7/7 (10.9 ± 0.9)	0/7	0/7	+4.5
B $F_{2\alpha}$	50–100 µg	0/6	6/6*	6/6	4/6 (3.6 ± 1.5)	+2.3
C $F_{2\alpha}$ + progesterone	100 µg } 4 mg }	4/4	4/4*	0/4	0/4	+6.9
D Progesterone	4 mg	4/4	4/4 (11 ± 1)	0/4	0/4	+4.5
E Control hysterectomy	0	—	3/3 (11 ± 0.9)	0/3	0/3	+3.1
F Hysterectomy + $F_{2\alpha}$	100 µg	—	1/4 (8)	3/4	3/4 (2 ± 0.6)	+1

* Could not be accurately counted.

0.2 ml. oil) from days 4–7 maintained pregnancy (group C). The embryos seemed to be normal in appearance and number, although the corpora lutea had degenerated. The other possibility is that the progesterone acted by rendering the uterine muscle insensitive to the contractile action of $F_{2\alpha}$; but this would not account for the degeneration of the corpora. Injection of progesterone alone, on the same schedule failed to produce luteal degeneration (group D).

Thus it seems that the degeneration of corpora did not involve the uterus. This was confirmed using hysterectomized animals. Injection of $F_{2\alpha}$ beginning the day of hysterectomy again caused luteal degeneration in all hamsters and ovulation (confirmed by the finding of tubal ova group F) in three out of four animals. Hysterectomy alone had no such effects in the control animals (group E). Similar results have been reported for guinea-pigs⁴.

The occurrence of ovulation during or after treatment with $F_{2\alpha}$ has not been reported before. If the PG did not make the ovaries hypersensitive to LH, then ovulation in the treated hamsters, apart from indicating that follicles were not damaged by the treatment, suggests an antecedent surge of LH. This ovulatory surge may have been the consequence of a deficiency of progesterone stemming from luteal degeneration. Alternatively, $F_{2\alpha}$ may have stimulated specifically the secretion of LH from the pituitary gland (as has been postulated earlier for the rat)⁵, the gonadotrophin then inducing both luteolysis and ovulation.

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***In vitro* Inhibition of Bovine Serum and Pancreatic Amylase by Progesterone**

THE recent attention given to the use of sex hormones for contraception has stimulated biochemical and physiological investigations of the mechanism of this effect. Experiments by

Jensen *et al.*¹ and Stone² have established that the vagina and ovaries (target tissues) are especially sensitive to these hormones. On the other hand, studies of Fiegelson³ and of Turkington and Hill⁴ with liver histidase and mammary gland lactase synthetase respectively, have indicated that sex hormones affect the activity of enzymes beyond the target tissue. Further evidence suggests that amylase and the secretion from the pancreas are influenced by dietary factors⁵ which in turn may be partially affected by steroid sex hormones. Whether the steroid hormones play a role in the control mechanisms of target as well as other tissues, by changing enzyme activity, is not known. We have investigated the effect of testosterone, 17- β -oestradiol and progesterone (Sigma) on pancreatic and serum amylase activity *in vitro*. Cholesterol (Merck) with no known sex hormone activity was used as control.

Bovine pancreatic powder ('Pancreatine', BDH) was extracted for proteins and the extract was used as a source of amylase (crude extract). Purified enzyme was prepared by extracting proteins from BDH pancreatic powder for 24 h (2 g/50 ml.) in distilled water at 4° C. Absolute ethanol, to a final concentration of 60% was added and the mixture was kept for 3 h in a refrigerator. The precipitate was centrifuged and the supernatant was saved. Additional cold absolute ethanol was added to give a final concentration of 75%. The mixture was kept at 4° C for another 3 h, after which the precipitate was again removed by centrifugation. This preparation was used for analysis (purified enzyme or PPE). Assay showed the latter preparation to be ten times more active per mg of protein, compared with the crude extract, or between 100 and 200 U/100 ml. representing 0.8–1 U/assay. For the analysis of bovine serum, amylase was assayed by pooling sera from several healthy bulls which were obtained from the local slaughter house.

In all cases amylase was assayed by the modified method of Smith and Roe⁶ as described by Khayambashi and Lyman⁸. Activity is expressed as a change in colour as measured at 620 nm by a Zeiss PQM II spectrophotometer in 15 min incubation at 37° C, per mg of protein. Protein in solution was estimated by the method of Warburg and Christian¹⁰ using the ratio of absorbance at 260 and 280 nm. Because the steroids tested were relatively insoluble in the assay solution, they were first dissolved in absolute ethanol and then added to the assay mixture to give a final concentration of ethanol in the assay solution of 1.62%. Solubility of each steroid in the aqueous media was measured by placing the steroid in the assay mixture, allowing it to stand at 37° C for 15 min and determining the undissolved steroid by filtration through a 'Millipore' filter (0.22 µm pore size). Cholesterol was determined by the method of Zlatkis *et al.*¹¹; progesterone, testosterone and 17- β -oestradiol concentrations were estimated by measuring absorbance at 240, 240 and 230 nm respectively.

Although the steroids to be tested for anti-amylitic activity were dissolved in ethanol, no appreciable change (maximum 20% compared with water) in enzyme activity was noted when 0.21 ml. of absolute ethanol was added in a total volume of 13 ml. of reaction mixture. In the solubility experiments,

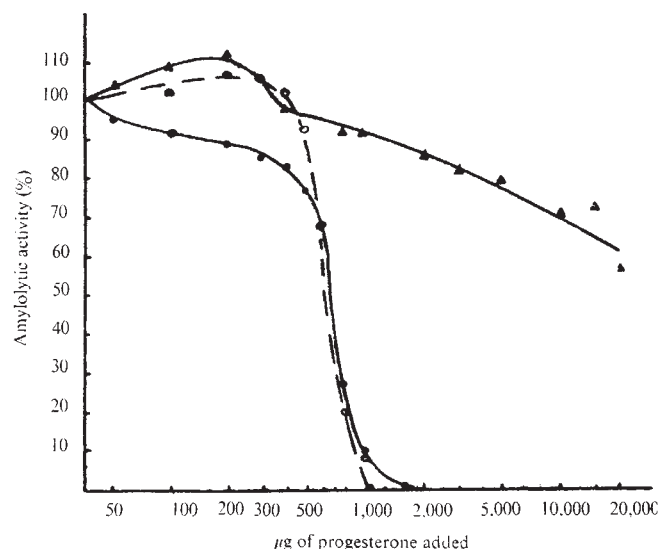


Fig. 1 Semilogarithmic plot of the percentage activity of bovine serum and pancreatic amylase as a function of progesterone added. Each point represents the average values of between five and eleven experiments with: ▲, serum; ○, crude (CPE) and ●, purified (PPE) pancreatic extracts. The average deviations were, respectively, ± 12 , ± 10 and $\pm 8\%$ and the corresponding total reaction volumes were 1.6, 13 and 13 ml.

cholesterol and progesterone reached saturation at 250 $\mu\text{g}/\text{ml}$. of added steroid. At this concentration, 3.75 μg of each added steroid was detectable in solution. Testosterone and 17- β -oestradiol were much more soluble in the enzyme reaction mixture, plus 0.21 ml. of alcohol, as the following comparison shows. When 75 $\mu\text{g}/\text{ml}$. of each steroid was added to the reaction mixture the following amounts, in μg , were detected in solution: cholesterol, 2.2; progesterone, 1.5; testosterone, 36.5; and 17- β -oestradiol, 8.

The effect of progesterone on serum as well as crude and purified (PPE) pancreatic bovine amylase is shown in Fig. 1. Only at high concentrations of progesterone was amylolytic activity of serum affected—maximum inhibition of 50% at 20 mg/ml. when the bovine serum enzyme activity was 1.1–1.2 U. In contrast to serum using bovine pancreatic amylase (crude or PPE) 95–99% inhibition of amylolytic activity was observed when 200 μg of progesterone/ml. of extract was added (each ml. of enzyme solution exhibited 0.55 ± 0.05 U of amylolytic activity and 2 μg protein). At a low concentration of progesterone crude enzyme activity was stimulated; at higher concentrations it was inhibited. The stimulation disappeared when the extract (PPE) was purified, however.

In contrast to progesterone, cholesterol exhibited no inhibiting enzyme activity, nor did testosterone or 17- β -oestradiol below a concentration of 560 μg of added steroid/ml. of solution (Table 1). At higher concentrations, enzyme activity was slightly inhibited, but in no instance did it exceed 10%. Increasing the concentration of steroid much beyond the level shown was not practical because of saturation of the solution and precipitation of the steroids.

The data presented here demonstrate a complete inhibition of bovine pancreatic amylase activity when progesterone is added. Whether crude or partially purified pancreatic extracts were used as the enzyme source, the effect was similar. One unit of pancreatic amylase activity was completely inhibited by the addition of 3–4 μmol of progesterone. At this concentration, it can be seen from solubility measurements that only about 2% of all progesterone added to the solution was dissolved. Thus a small amount of the steroid (less than 0.1 μmol) caused complete inhibition.

Although progesterone strongly inhibited pancreatic and partially inhibited serum amylase, other steroids tested had no significant effect on amylase from either source. Only

Table 1. Effect of Cholesterol, Testosterone and 17- β -Oestradiol on the Amylolytic Activity of Bovine Serum and Pancreatic Extracts

Steroid (μg) *	Cholesterol Serum	Cholesterol †PPE	Testosterone Serum	Testosterone PPE	17- β -Oestradiol Serum	17- β -Oestradiol PPE
0	100	100	100	100	100	100
50	—	—	—	—	100 $\pm$ 4	104 $\pm$ 1
70	98 $\pm$ 6	100 $\pm$ 4	—	—	—	—
100	—	—	106 $\pm$ 9	99 $\pm$ 3	106 $\pm$ 2	106 $\pm$ 2
140	103 $\pm$ 10	102 $\pm$ 4	—	—	—	—
150	—	—	—	—	95 $\pm$ 1	98 $\pm$ 3
200	—	—	104 $\pm$ 6	98 $\pm$ 5	94 $\pm$ 1	100 $\pm$ 4
210	98 $\pm$ 6	99 $\pm$ 2	—	—	—	—
280	95 $\pm$ 4	95 $\pm$ 5	—	—	—	—
300	—	—	104 $\pm$ 7	97 $\pm$ 4	94 $\pm$ 5	88 $\pm$ 2
400	—	—	98 $\pm$ 8	96 $\pm$ 1	96 $\pm$ 6	91 $\pm$ 2
420	100 $\pm$ 8	93 $\pm$ 7	—	—	—	—
500	—	—	—	100 $\pm$ 1	96 $\pm$ 3	88 $\pm$ 2
560	97 $\pm$ 7	88 $\pm$ 5	—	—	—	—
600	—	—	102 $\pm$ 8	93 $\pm$ 3	—	—
700	98 $\pm$ 6	85 $\pm$ 6	—	—	88 $\pm$ 9	—
800	—	—	106 $\pm$ 5	90 $\pm$ 2	—	—

* The amount of steroid which was added to 0.8 ml. of serum, or 2 ml. of pancreatic extract in 13 ml. of the reaction mixture⁸.

† PPE = Purified pancreatic extract.

17- β -oestradiol depressed enzyme activity to a small extent (maximum 20%).

In essential agreement with the results of Rao, who used salivary amylase⁶, bovine serum exhibited a maximum of 50% inhibition. No additional change was observed with serum even at very high concentrations of progesterone (some twenty times higher than the maximum used with pancreatic extracts). At the maximum concentration of progesterone used (65 $\mu\text{mol}/\text{U}$) presumably all of the steroid-carrying proteins were occupied¹³, leaving some progesterone in the free form. Nevertheless, complete inhibition of enzymatic activity was not observed with serum. At concentrations higher than 65 $\mu\text{mol}/\text{U}$, larger amounts of ethanol were required to solubilize the progesterone. These higher concentrations of ethanol caused increasing loss of enzyme activity; they were therefore not tested. The incomplete inhibition of serum amylase as contrasted to the complete inhibition of pancreatic amylase by addition of progesterone, may be explained partly by the fact that presumably the former amylase could be derived from various sources. For amylase several isozymes have been identified⁵ which could vary in susceptibility to inhibition by progesterone.

The inhibitory effect of some steroids on enzymes other than pancreatic or serum amylase have been shown before. Hayano and Dorfman⁷ using desoxycorticosterone showed that amylase was slightly inhibited. D-Amino-acid oxidase was only partially affected by progesterone and was more strongly inhibited by DOC¹³. Other enzymes such as lactase synthetase⁴, histidase³, tryptophan oxygenase were also affected by steroids *in vivo* or *in vitro*¹⁴. Furthermore, the interaction between glutamic acid dehydrogenase and steroids¹⁵, as well as the observations we have described here, all suggest that steroids are involved in metabolic control systems. It is possible that this effect (enzyme-steroid interaction) can explain some physiological cellular and organ phenomena—for example, the influence of androgens on gastric secretion of guinea-pigs¹⁶ and on ribosome synthesis¹⁷.

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Binding of Sheep Red Blood Cells to a Large Population of Human Lymphocytes

WHILE investigating the interaction between antigen-antibody-complement complexes and lymphoid cells¹, we have noticed that sheep red blood cells (SRBC) adhered to a surprisingly large proportion of human peripheral blood lymphocytes forming clusters ("rosettes"). We have now examined the conditions in which this interaction occurs, and the frequency of rosette forming lymphocytes in the peripheral blood of normal individuals and a few patients with haematological diseases.

Human lymphocytes were obtained from heparinized blood by passage through nylon wool², washed twice by centrifugation at 200g and resuspended in Hanks's balanced salt solution (HBSS) to a concentration of 2×10^6 /ml. This suspension, which contained more than 90% lymphoid cells, was mixed with an equal volume of a 0.5% suspension of SRBC in HBSS. SRBC were stored in isotonic citric acid-dextrose solution and used within 2 weeks of bleeding. The tubes containing the mixture were centrifuged at room temperature for 5 min at 200g and then incubated in an ice bath for 60 min. The tubes were then gently rocked to resuspend the cells in the pellet and the rosettes were counted in a haemocytometer. A rosette was defined as a lymphocyte with at least three red cells adherent to its membrane. Most clusters, however, contained between five and ten SRBC. The control tubes, with SRBC alone, never contained any clusters.

The binding of SRBC to lymphocytes was dependent on temperature. Maximum adherence occurred when first contact between SRBC and the lymphoid cells was established at 37° C or at room temperature and was followed by an additional period of incubation at low temperature. As Fig. 1 shows, the clusters increased with time of incubation at low temperature and reached a plateau after 60 min. These rosettes promptly disappeared if the tubes were reincubated at 37° C, but reassociation occurred after centrifugation and incubation at low temperatures. On the other hand, no rosettes formed if all the steps of the reaction were carried out in an ice bath or at 37° C. Thus it seems that there are stringent temperature requirements for the two stages of the reaction; the first, when contact between cells is probably established, does not take place at low temperature, while aggregation or

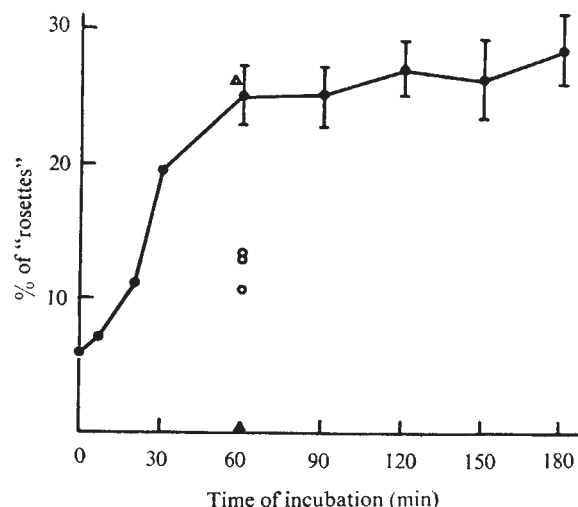


Fig. 1 Percentage of clusters formed when SRBC were incubated with human peripheral blood lymphocytes in an ice bath for different times. The points from 60 to 180 min represent the means ($\pm$ s.e.m.) of the results obtained with lymphocytes from twenty-two subjects (20–50 yr old, males and females). Thoracic duct lymphocytes were obtained from a patient before kidney transplantation. Burkitt lymphoma cells were from established cultures maintained by repeated passage in the laboratory. One of them (R69–517) was on its twenty-third passage after isolation. ●, Normal individuals; △, thoracic duct; ○, acute lymphatic leukaemia; ▲, Burkitt lymphoma.

cluster formation may occur at low temperature, but not at 37° C.

Among human peripheral blood leukocytes, only lymphoid cells bound SRBC. This could be ascertained as follows. Leucocyte-rich plasma was collected as described, but without passage through nylon wool. The leucocytes were washed by centrifugation at 200g with HBSS, mixed with SRBC, centrifuged, and after appropriate incubation thin smears were prepared and stained. Although lymphocytes constituted less than 40% of the white cells of the smears, no rosettes were found on monocytes or polymorphonuclear cells. SRBC also bound to human lymphocytes from the thoracic duct and thymus. In five cases in which it was possible to study lymphocytes from the thymus and peripheral blood of the same donor, a significantly higher percentage of rosettes was found among thymocytes (in one case almost 100%). Among normal adults, between 20 and 40% of lymphocytes formed rosettes when incubated with SRBC. The data in Fig. 1 were obtained with cells from twenty-two normal individuals, but peripheral blood lymphocytes from more than thirty others (Caucasians, Negroes and Mongoloids) were tested with similar results. The proportion of lymphocytes binding SRBC was significantly diminished in three cases of acute lymphatic leukaemia, but seemed normal in individuals with chronic lymphatic leukaemia, Hodgkin's disease and lymphosarcoma. No rosettes were observed when we used cells obtained from cultures of Burkitt lymphoma cells (lines Jiyoye and R69–517).

This interaction is species specific in relation to the erythrocyte and lymphocyte. Only a few lymphoid cells obtained from lymph nodes or spleens of other mammalian species (rabbit, guinea-pig, rat and mouse) formed rosettes when incubated with SRBC in the same conditions (0–0.5%). Furthermore, human lymphocytes did not bind red cells from other species tested (horse, ox, rabbit, guinea-pig, rat, mouse or human red cells of blood groups A, B and O). A low but significant degree of binding was detected, however, between human lymphocytes and pig red blood cells (PRBC). Fewer clusters were obtained with PRBC than with SRBC, and when both kinds of red cells were incubated simultaneously with human lymphocytes many mixed rosettes were observed. Only rare clusters contained exclusively PRBC or SRBC. This

observation indicates that a unique population of human lymphocytes is involved, and that perhaps the same receptor binds both kinds of red cells.

This observation is an example of an interaction between a lymphoid cell and a heterologous erythrocyte which, although having some degree of specificity, is probably not immunological in nature for several reasons. First, a very high proportion of peripheral blood lymphocytes bind SRBC. It would certainly be difficult to admit that 20–40% of the lymphocytes have anti-SRBC antibodies on their membranes. Second, many of the donors had “natural” antibodies in the serum directed against SRBC or PRBC, but no correlation could be established between the titres of antibodies and the percentage of rosettes found among their lymphocytes. A patient with mononucleosis was tested while her titre of anti-SRBC in the serum was still high (1:112) but the percentage of rosettes was in the normal range (24.6%). Third, although the same population of human lymphocytes bound both PRBC and SRBC, serum antibodies against these kinds of erythrocytes did not cross react, as could be determined by absorption experiments. Fourth, this phenomenon has peculiar temperature requirements, which are different from those reported for cluster formation between erythrocytes and lymphoid cells from immunized animals³.

Another implication of these findings is that SRBC or PRBC may interact with human lymphoid cells when they are cultivated together *in vitro*. Actually a low grade incorporation of isotopically labelled thymidine has been reported in such cultures^{4,5}, which was abolished if the erythrocytes were disintegrated by ultrasound. The observation described here may also be relevant to the findings of Bach *et al.*^{6,7}, who noticed that a few human peripheral blood lymphocytes (0.4–2.6%) form rosettes when incubated with SRBC. They found that this interaction is inhibited by anti-lymphocytic sera and suggested that the immunosuppressive potency of these sera could be measured by their rosette inhibitory titres. The experimental conditions used by Bach *et al.* differ from those here described, and so the relationship between their findings and ours remains to be clarified. Finally, because only part of the circulating lymphocytes are involved in cluster formation, it would be important to establish whether they constitute a separate sub-population of cells, as suggested by the larger proportion of clusters among thymocytes and their absence among some cultivated Burkitt lymphoma cells.

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Note added in proof. Two papers have recently been published describing the same phenomenon: Brain, P., *et al.*, *Clin. Exp. Immunol.* 6, 681 (1970); Coombs, R. R. A., *et al.*, *Int. Arch. Allergy*, 39, 658 (1970).

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Effect of N,N-Dimethyltryptamine and D-Lysergic Acid Diethylamide on the Release of 5-Hydroxyindoles in Rat Forebrain

SEVERAL investigators have already shown that electrical stimulation of the mid-brain raphe, in which neuronal perikarya containing 5-hydroxytryptamine (serotonin) are almost exclusively located¹, produces a significant increase in the forebrain content of 5-hydroxyindolyl-3-acetic acid (5-HIAA), while the concentration of serotonin decreased or remained unchanged^{2–6}. This communication describes how N,N-dimethyltryptamine (DMT) and D-lysergic acid diethylamide (LSD), the two potent psychogenic drugs, affect the release of serotonin and 5-HIAA in the forebrain when the mid-brain is stimulated. Both drugs uniformly inhibit the response of single units in the mid-brain raphe nuclei^{7,8}.

Adult rats of both sexes (180–250 g) were anaesthetized lightly with intraperitoneal urethane (1.0 g/kg of weight), and a bipolar steel stimulating electrode was placed stereotactically in the caudal mid-brain raphe⁹ in stimulated and unstimulated rats at A (anterior) 0.4 mm; L (lateral) 0.0 mm and H (horizontal)—2.6 mm. The correct placement of each electrode was confirmed histologically. Stimuli were monophasic square wave pulses (10 Hz, 2 ms in duration, 4–6 V) applied for 40 min from a Grass S8 stimulator. N,N-dimethyltryptamine (Sigma) was administered intraperitoneally (36 mg/kg) at the start of stimulation. LSD (0.6 mg/kg, Sandoz) was given intraperitoneally in two equal doses; the first at the onset of stimulation and the second 20 min after the start of stimulation. This mode of administration was chosen because the effect of LSD on the metabolism of serotonin correlates with the time course of clearance of the drug from the brain. At corresponding times, control animals received equal volumes of saline. After the cessation of stimulation, rats were decapitated and the forebrains were removed immediately and frozen at –20° C. The serotonin and 5-HIAA content of the forebrains was measured spectrophotofluorimetrically within 24 h¹⁰, results were analysed statistically by Student's *t* test.

In agreement with previous results^{11–14}, we found that LSD and N,N-dimethyltryptamine produce a significant ($P < 0.05$) increase in the serotonin in the rat brain and a decrease in the 5-HIAA (Figs. 1 and 2). Both drugs significantly ($P < 0.05$) reduced the increase in forebrain 5-HIAA regularly seen in untreated animals after raphe stimulation. In stimulated rats, however, LSD produced a small but significant ($P < 0.05$) decrease in the forebrain concentration of serotonin (Fig. 1), while there was an increase in this compound ($P < 0.025$) in animals pretreated with DMT (Fig. 2). DMT interacted more consistently with the induced increase in forebrain 5-HIAA than LSD. Our results obtained with LSD do not agree with data reported by Kostowski and Giacalone¹⁵.

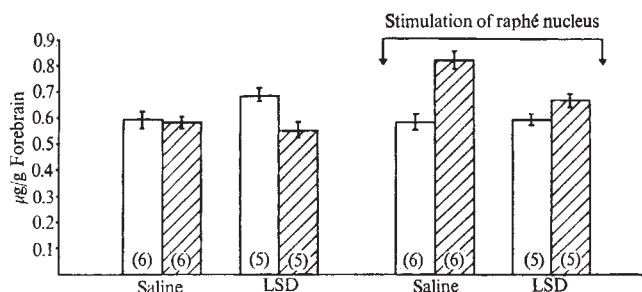


Fig. 1 Concentrations of serotonin (white columns) and 5-HIAA (hatched columns) in the forebrain with and without electrical stimulation of mid-brain raphe in control (saline) and LSD-pretreated rats. Values shown are the means ($\pm$ s.e.) of five and/or six determinations (the exact numbers of animals are shown on the corresponding columns).

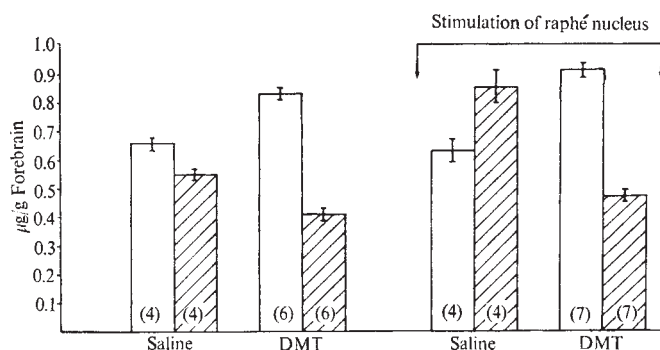


Fig. 2 Concentrations of serotonin (white columns) and 5-HIAA (hatched columns) in the forebrain with and without electrical stimulation of mid-brain raphe in control (saline) and DMT-pretreated rats. Values shown are the means ($\pm$ s.e.) of four, six and/or seven determinations (the exact numbers of rats are shown on the corresponding columns).

Although the effects of LSD and DMT on brain serotonin metabolism¹¹⁻¹⁴ and on the activity of serotonin-containing neurones in raphe nuclei⁸ are similar, the actions of the drugs on stimulation-induced serotonin release differed in our experiments; LSD decreased the concentration of serotonin in the forebrain, while DMT increased it. As to the possible mechanism of interaction of LSD and DMT with the altered concentrations of serotonin and 5-HIAA in the forebrain caused by raphe stimulation, several factors such as release, uptake, storage, binding, monoamine oxidase activity, drug interference with specific receptors and inhibition of neuronal activity have to be considered. Diaz *et al.*¹³ found that LSD slowed the turnover of serotonin in the brain and suggested that this was a consequence of a blockade of serotonin release similar to that reported for noradrenaline in peripheral sympathetic neurones¹⁶. Our experiments suggest that LSD has an inhibitory effect on biosynthesis and/or the release of serotonin from nerve terminals, which would lead to a decrease in brain serotonin turnover and inhibition of serotonin-containing neurones. This hypothesis is supported by the finding that LSD blocks the release of ³H-serotonin induced by electrical stimulation of brain slices¹⁷. As for DMT, it is believed to be a potent inhibitor of the uptake of serotonin and the effect of raphe stimulation on the serotonin and 5-HIAA in the rat forebrain might be explained on this basis. If the serotonin released by stimulation were not taken up and metabolized by monoamine oxidase, this would account for the observed increase in the forebrain serotonin content. Further, DMT may interfere with the release of serotonin.

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3-O-Methyldopa, a New Precursor of Dopamine

3-METHOXY-4-HYDROXYPHENYLALANINE (3-O-methyldopa) is a major metabolite of L-3,4-dihydroxyphenylalanine (L-dopa) in animals and man¹⁻⁴ and when labelled it easily penetrates from the blood into other tissues, for example the brain and heart, where it has a biological half life of 12-13 h, which is considerably longer than that of L-dopa, which is about 30 min. The bulk of the urinary radioactivity excreted within 24 h consists of homovanillic acid (3-methoxy-4-hydroxyphenylacetic acid) and 3-methoxy-4-hydroxyphenyllactic acid⁵.

We have identified and measured other minor metabolites of L-3-O-methyldopa in the urine, and investigated the effect of enzyme inhibitors on the metabolism of this amino-acid in the brain. We wish to report evidence that L-3-O-methyldopa is partially demethylated in the organism.

Rats of both sexes were given L-2-¹⁴C-3-O-methyldopa (specific activity 193 μ Ci/mg) intraperitoneally. Some of the animals were pretreated with various inhibitors of catecholamine metabolism. The urine was collected for 4 h and incubated with 'Glusulase'. Brain homogenates were deproteinized and submitted to column chromatography to separate amino-acids, catecholamines and phenolcarboxylic acids, which were isolated by paper chromatography and measured for radioactivity in a liquid scintillation counter as described before^{3,5}.

About 6% of the administered radioactivity was found in the urine. Table 1 shows that about 8.5% of the excreted radioactivity was in the dihydroxylated metabolites dopa, dopamine and 3,4-dihydroxyphenylacetic acid. About 74% of the metabolites, however, consisted of 3-methoxy-4-hydroxy-

Table 1 Radioactive Metabolites of L-2-¹⁴C-3-O-Methyldopa in the Urine

Demethylated metabolites	
Dopa	0.62 $\pm$ 0.01
Dopamine	3.35 $\pm$ 0.06
3,4-Dihydroxyphenylacetic acid	4.64 $\pm$ 0.68
Methylated metabolites	
3-O-Methyldopa	4.68 $\pm$ 0.01
3-Methoxytyramine	2.05 $\pm$ 0.06
Homovanillic acid	45.45 $\pm$ 1.25
3-Methoxy-4-hydroxyphenyllactic acid	26.37 $\pm$ 1.32
Unidentified acidic metabolites	12.27 $\pm$ 0.72

Metabolites were collected 4 h after intraperitoneal administration of 1 mg/kg L-2-¹⁴C-3-O-methyldopa to rats. The values are expressed in percentage of total radioactivity found in the urine, and represent averages with s.e. of two to eight experiments each with pooled urines from six rats. The total radioactivity excreted in the urine in the first 4 h of the experiment amounted to 6.4 $\pm$ 0.8% of the total injected radioactivity.

Table 2 Amine Fraction recovered from Brain of Rats treated with 10 mg/kg L-2-¹⁴C-3-O-Methyldopa and killed 2 h Later

Pretreatment	Total amine fraction *	Dopamine	Percentage composition of amine fraction 3-Methoxytyramine	Noradrenaline	Normetanephrine
None	0.06 ± 0.01				
Pargyline	0.29 ± 0.04	16.6 ± 0.3	39.9 ± 0.4	34.5 ± 1.4	9.1 ± 1.6
Pargyline + dopacetamide	0.60 ± 0.02	20.9 ± 2.4	Nil	79.1 ± 2.4	Nil

* In % of total radioactive compounds in brain.

150 mg/kg pargyline have been administered 3.5 h and 400 mg/kg dopacetamide 0.5 h before L-3-O-methyldopa. All compounds were given intraperitoneally. Average with s.e. of two to four duplicate experiments each with a pool of three to four brains.

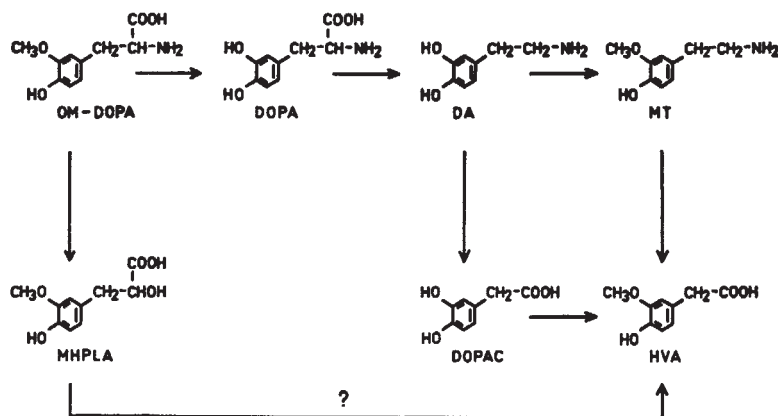


Fig. 1 Proposed metabolic pathways of L-3-O-methyldopa in brain. OM-dopa, 3-O-methyldopa; DA, dopamine; MT, 3-methoxytyramine; MHPLA, 3-methoxy-4-hydroxyphenyllactic acid; DOPAC, 3,4-dihydroxyphenylacetic acid; HVA, homovanillic acid.

derivatives, that is homovanillic acid, 3-methoxy-4-hydroxyphenyllactic acid and 3-methoxy-tyramine. About 5% of 3-O-methyldopa was also found. The rest of the radioactivity (about 12%) could not be identified.

In the brains of rats treated with L-3-O-methyldopa alone, only 0.06% of the total radioactive compounds was found in the amine fraction after 2 h. We did not separate this fraction into individual metabolites. Inhibition of oxidative deamination by pargyline markedly enhanced the L-3-O-methyldopa-induced increase in the amines—dopamine, noradrenaline and their corresponding 3-O-methylated derivatives. Dopacetamide (3,4-dihydroxyphenylacetamide), an inhibitor of catechol-3-O-methyltransferase, in combination with pargyline further increased the amine fraction and markedly changed its composition. There was a considerable rise in the dihydroxylated amines, in particular noradrenaline and the methoxylated catecholamines decreased to unmeasurable amounts (Table 2).

These results demonstrate that in the organism, including the brain, L-3-O-methyldopa is partly demethylated, for example to dopamine. Formation of large amounts of noradrenaline after administration of L-3-O-methyldopa alone does not seem likely, for neither the amine nor its metabolites have been detected in the urine. The appearance of noradrenaline in the brain of the animals pretreated with pargyline may be the consequence of a shift in dopamine metabolism. Thus, inhibition by pargyline of oxidative deamination, a major metabolic pathway of dopamine might lead to enhanced dopamine-β-hydroxylation.

The results with dopacetamide in combination with pargyline show that the appearance of cerebral 3-methoxytyramine is probably not due to a direct decarboxylation of L-3-O-methyldopa, but rather to 3-O-methylation of dopamine, which has been formed from L-3-O-methyldopa by demethylation and subsequent decarboxylation. In fact the inhibition of catechol-3-O-methyltransferase caused the 3-O-methylated amines to disappear and increased the amounts of catecholamines. This would not be the case if 3-methoxytyramine were a product of direct decarboxylation of L-3-O-methyldopa. It is therefore likely that at least part of the cerebral homovanillic acid found after administration of L-3-O-methyldopa⁵ is also derived from dopamine by oxidative deamination and 3-O-methylation. Preliminary experiments with urine of animals pretreated with dopacetamide confirm this view. Accordingly, the inhibitor

decreased the L-3-O-methyldopa-induced excretion of homovanillic acid and markedly enhanced that of dihydroxyphenylacetic acid without diminishing the concentration of 3-methoxy-4-hydroxyphenyllactic acid. But some formation of homovanillic acid from 3-methoxy-4-hydroxyphenyllactic acid or its precursor 3-methoxy-4-hydroxyphenylpyruvic acid, which probably originate from L-3-O-methyldopa by transamination, cannot be excluded.

The 3-O-methylated catechol derivatives (for example, homovanillic acid) found in brain and urine probably derive in part from dopamine, and so the amounts of this amine formed from L-3-O-methyldopa must be considerably greater than those actually found.

Metabolic pathways for L-3-O-methyldopa based on these results are proposed in Fig. 1.

Accumulation of 3-O-methyldopa in the brain and its slow conversion to dopamine may explain why the therapeutic effect of L-dopa in patients with Parkinson's syndrome disappears only slowly (within days or weeks) after discontinuation of treatment. This cannot be explained by the presence of dopa in the brain because of its very short biological half life. Preliminary studies indicate that L-3-O-methyldopa is metabolized by man in much the same way as in animals and exerts a therapeutic action in Parkinson's syndrome without, so far, any evidence of side effects (I. K., G. B., R. Tissot, and A. P., in preparation, and personal communication from R. Tissot).

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BOOK REVIEWS

God and the Machine

Science and Secularity: The Ethics of Technology. By Ian G. Barbour. Pp. 151. (Harper and Row: New York, Evanston and London, 1970.) \$4.95.

ON reading through the references given at the back of this book I realize how many texts have been written in the last decade on the complex relationship between man and machine. This no doubt reflects the increasing awareness that science in the form of power is at present unable to serve the best interests of society.

This book is the third that Dr Barbour, who is chairman of the Religion Department and Professor of Physics at Carleton College, has written on this subject; the other two have the titles *Issues in Science and Religion* and *Christianity and the Scientist*. It is based on a series of lectures given by the author at Duke University and the Pacific School of Religion, Berkeley. The principal part of the book is devoted to an examination of how contemporary religion stands up to the challenges of science; the first three chapters deal with the challenges from general characteristics of science, the scientific method, autonomy of nature and the technological mentality, while the following two chapters are concerned with the specific problems arising from molecular biology and cybernetics.

The opening chapter on the scientific method challenges the idea that science is objective and religion subjective and compares the approach of science and religion to the interpretation of experience. The role of models in scientific discovery is already well documented but it is as well to realize that religion, as well as science, uses models to interpret phenomena and experiences. Examples of scientific models are given as well as models of God taken from classical theology and from contemporary religious thought. Too often today we find the laws of physics and chemistry being applied to other fields without concern for the assumptions implicit in their derivation. The limitations of the models as representations of reality are, however, emphasized whether they be models of the nucleus or models of God. The differences between models in religion and science are considered and the author concludes that the challenge of

the scientific method can be met by a more adequate understanding of the character of both science and religion.

Secularism is a faith in man's ability to order his own life and control his own destiny. With this belief the world can be understood by man's independent reason without reference to God; this is where science has influenced secularity. The chapter on the autonomy of nature shows how religious thought through the ages has attempted to interpret God's relation to nature. This section of the book is not light reading, but it shows how contemporary religion recognizes the concerns of secularity with its belief in the autonomy of nature and the freedom of man, and puts forward the model of a God of persuasion. "God's power is the power of love which evokes man's response but does not compel."

Barbour is not alone in being concerned with the emergence of the technological mentality with its reliance on the power of technology and its shameless exploitation of the environment. He sees it as a challenge to religion in that it makes a God out of technology, with growth and continued expansion as the objects of worship. As he rightly points out, technology has the power to free man from suffering, disease, famine and poverty; but its misuse can lead to untold misery. The author calls for a change of attitude and pleads for a technology which takes into account human needs.

The so-called "new" challenges of molecular biology and cybernetics are discussed in terms of the philosophical, theological and ethical issues that they present. Genetic control has the power for good in improving mankind, cybernetics can extend man's capacity and augment his intellectual power. Barbour uses these two subjects to illustrate that the new freedom is coupled with the needs for greater responsibility and his model of man includes that of a responsible self as well as the biochemical system that he is.

In the chapters on the technological mentality, biochemical man and cybernetics, the red warning light has been flashing. In the final chapter on the redirection of technology a green arrow is also visible, and he stresses that technological suicide is not inevitable; man has the choice. At this moment in history, the world faces a crisis of values and in his suggestions as to how

to approach the problems one concludes that Barbour has a very balanced view of the nature of man. It may well be fashionable to talk about ecological crises and the population explosion, especially to an American audience, but these are not idle words. Concrete proposals for dealing with the population crisis are set out coupled with a plea to the countries of the world to adopt a national policy on the redirection of technology towards the pressing social problems. It is not enough for scientists, however distinguished, to voice their dissatisfaction with the unbridled progress of technology; government interference is needed to restore the concept of man in nature rather than man against nature.

The Christian Church does not escape criticism. Up to now the Church has resisted change but it must "assist in directing technology and also make us aware of the dimensions of life inaccessible to technical reason". "The gospel is not the enemy of human freedom and fulfilment but liberates us to discover our authentic humanity."

This book aids the scientist to see his work in perspective and helps the layman to understand how the rot has set into technological development. It is indeed a positive contribution to the guide book of survival in the seventies.

KEITH N. JONES

Origins of "New Science"

Science and Education in the Seventeenth Century. (The Webster-Ward Debate.) By Allen G. Debus. (History of Science Library: Primary Sources.) Pp. 335. (Macdonald: London; American Elsevier: New York, November 1970.) £6.00.

ALTHOUGH the introduction of "New Science" in place of the Aristotelian-scholastic natural philosophy at English universities had been urged since the publication of Francis Bacon's *The Advancement of Learning* in 1605, such a far-reaching change seemed close to realization only in the heady atmosphere of the English Commonwealth. Historians of education have often commented on the pleas for a new scientific teaching which constituted an important part of the many reformist proposals (some of extraordinary perceptiveness) published at the time.

Only recently has it become clear that the "New Science" of many of

these sectarian reformers was not the revolutionary mechanical-mathematical approach of Galileo and Descartes, but a Renaissance Hermeticism which, in an influential Paracelsian version, claimed to solve all problems in natural philosophy through a universal chemistry closely tied with religious-social motives for the study of God's creation and making discoveries for human benefit. That explains the seeming paradox of the most eloquent and closely-argued of such proposals, John Webster's *The Examination of Academies* (1653), being answered not by an old-fashioned Aristotelian but by Seth Ward, Savilian Professor of Astronomy at Oxford, and a member of the group of young natural philosophers who formed the nucleus of the Royal Society which was to be established soon after the Restoration. Ward's polemic lacks the verve and excitement of Webster's prose, but when read together the two works convey a vivid sense of the struggle between rival ideals of a "New Science" to take the place of discredited Aristotelianism. The social and political overtones of that conflict are indicated by the reference to Webster's "New-found-light . . . tending to the subversion of Universities, Philosophers, Physicians, Magistrates, Ministers . . ." in the title of the third work in this collection.

Professor Debus's long introduction places the English debate in the context of the larger European debate on educational reform. Although expensively priced, the facsimile edition of these rare works should interest a wide circle of readers, and stimulate attempts to relate them more closely to the political, social, and educational context of the Commonwealth period.

P. M. RATTANSI

Fishes of Canada

Freshwater Fishes of Northwestern Canada and Alaska. By J. D. McPhail and C. C. Lindsey. (Fisheries Research Board of Canada Bulletin No. 173.) Pp. 381. (Queen's: Ottawa, 1970.) \$8.50.

ICHTHYOLOGISTS and fishery workers have been well served by the book publishing programme of the Fisheries Research Board of Canada. In 1946 (second edition 1961) it produced Clemens and Wilby's *Fishes of the Pacific Coast of Canada* (Bulletin 68), and in 1966 Leim and Scott's *Fishes of the Atlantic Coast* was published as Bulletin 155. Now the *Freshwater Fishes of Northwestern Canada and Alaska*, by J. D. McPhail and C. C. Lindsey, completes the trinity, and, with the exception of the eastern freshwaters of Canada, completes the coverage of

the country. No area of the size of Canada has a comparable ichthyological literature.

McPhail and Lindsey have produced an original work largely as a result of their own extensive researches in the area. The fish fauna here is of great interest on account of its relatively recent arrival, the simplicity of the freshwater communities and, above all, because of the minimal biological pollution it has suffered by the human introduction of exotic forms and the translocation of native species. There are only about 60 species of native freshwater fish in this area of some 2 million square miles. Thanks to the relatively sparse human population of the area this fauna has been little altered by abuse of aquatic resources, and distribution studies are meaningful in the sense that they make a real contribution to zoogeography. In addition, the imminence of large-scale industrial exploitation in this region adds a particular topicality to the authors' attempts to record the present distribution of the fish fauna.

The relatively recent glaciation of the region makes it singularly suitable for evolutionary studies. Its aquatic fauna survived glaciation in widely separated refuges around the periphery of the ice, and some diverged enough from ancestral forms in this isolation to make it possible to distinguish their descendants today. It thus becomes possible to suggest the routes of postglacial dispersal for many of the species. Broadly, the available refuges during the Wisconsin glaciation were the Bering area, the Pacific coast, the Missouri-Upper Mississippi, and the Atlantic coastal region. The north-western region has been colonized from the first three of these areas. On the basis of present distribution the authors show that eleven of the native fish entered the area from the Pacific refuge, ten from the Bering, and eighteen from the Mississippi refuge. The remaining twenty-one species survived in more than one refuge. These last are perhaps of the greatest interest to the evolutionist, and offer a solution to the puzzling variations noted between the populations of some species. Populations of such fish as the smelt (*Osmerus eperlanus*) and the three-spined stickleback (*Gasterosteus aculeatus*), which now occupy disjunct ranges, found sanctuary in the Atlantic and Bering refuges (smelt), and Atlantic and Pacific refuges (stickleback), but have not since mingled. On the other hand, the Arctic charr (*Salvelinus alpinus*), and the burbot (*Lota lota*) are examples of those fish which have reestablished a continuous range from two or more refuges, although slight morphological differences can still be detected in the populations and their intermediate forms.

The theme of zoogeography, with postglacial and present distribution given for each species is recurrent throughout this book. Moreover, it contains for each species a full description and figure, notes on useful distinguishing features, and most informative and interesting accounts of their biology.

ALWYNE WHEELER

Enzymes Alike

Isoenzymes. By J. Henry Wilkinson. Second edition. Pp. x+369. (Chapman and Hall: London, November 1970.) £3.75.

THE literature dealing with isoenzymes has been increasing so rapidly that it is virtually impossible for any single individual to produce a book dealing adequately with the whole field. This is well illustrated by *Isoenzymes*, although the author claims to have added over 1,000 new references. Incidentally, only twelve or so are dated later than 1968.

Including the introduction, this second edition has twelve chapters, of which three are new. The early sections deal in a very clear fashion with matters of nomenclature and techniques for separation, detection and determination. Although this part of the book covers a fairly wide field, it is not really very detailed. There is, for example, only brief mention of the use of isoelectric focusing.

The new chapter dealing with the chemical nature of isoenzymes covers somewhat superficially most aspects of this topic, including a brief account of conformational isomerism. There is, however, no account of duplicate isoenzymes. The author suggests that it would be attractive to propose that an enzyme might be associated with several non-enzymatic proteins with different migratory properties. He states, however, that he is not aware of any enzyme whose heterogeneity could be attributed to differences in the nature of inert carrier proteins. Nevertheless, in his account of macroamylasaemia, in a later chapter, there is a description of work which indicates that there is an association of amylase with normal serum immunoglobulins. It is also well known, but not mentioned in the text, that α_2 -macroglobulin combines with a number of enzymes and that this type of complex can account for at least one of the apparent isoenzyme forms of aspartate aminotransferase.

A second new chapter, dealing with enzyme multiplicity in the glycolytic pathway and the pentose-phosphate cycle, is quite a good, if not very detailed, account of knowledge in this field but, unfortunately, is somewhat marred by the diagram of metabolic pathways, which gives the impression

that erythrose 4-phosphate is an end product.

The longest of the chapters deals with isoenzymes of lactate dehydrogenase. This is fairly comprehensive. There is, however, no account of the quaternary structure of the lactate dehydrogenase of dogfish muscle, as determined by X-ray techniques. It is perhaps unfortunate that the author attributes to Langvad the discovery that the malignant type of shift in isoenzyme pattern occurs in the morphologically unchanged cells in the neighbourhood of malignant tumours. In actual fact this was described considerably earlier by British authors. In this chapter it is pointed out that the absence of the slowest moving isoenzyme of lactate dehydrogenase from red blood cells is apparently not consistent with random reassociation of enzyme subunits. This gives the totally erroneous impression that experiments done *in vitro* apply to the situation *in vivo*, although evidence has been available for some time that this is certainly not the case. The clinical sections of this chapter are undoubtedly too brief and would not be of great help to a clinician interested in differential diagnosis.

In the chapter dealing with phosphatase isoenzymes, there are a number of important omissions. There is insufficient discussion of the relationship of intestinal alkaline phosphatase to ABO blood groups and nothing about the effect of fatty meals. There is only the very briefest discussion of the Regan and other isoenzymes, which occur in the serum of cancer patients as well as in certain malignant tumours. The account of alkaline phosphatase isoenzymes in bacteria is far too brief.

The remaining new chapter on the biological significance of isoenzymes is grossly inadequate. There has been a good deal of work in this field particularly in relation to bacteria and plants which ought to have been included in a book described as a comprehensive review of the literature.

As was the case with the first edition, the way this book is arranged calls for a very detailed index. Unfortunately, in practice the very reverse is true and there are a number of significant omissions. For example, there is no reference at all to the discussion of fish lactate dehydrogenase isoenzymes on pages 172-173. There is also insufficient cross-referencing.

It is doubtful whether there has ever been a perfect book dealing with a scientific topic. Although there is much to criticize, it should be pointed out that this monograph is very lucidly written and contains much relevant information. It can be recommended to readers who are not already engaged in this field. It would not be of much use to the clinician.

A. L. LATNER

Trypanosome and the Fly

The African Trypanosomiases. Edited by H. W. Mulligan. Pp. lxxxviii + 950. (Allen and Unwin: London, March 1971.) £10.00.

THE problems of trypanosomiasis in Africa concern many disciplines. Parasitology, entomology, public health, veterinary science, agriculture and even tourism are all involved. Since the European penetration of Africa in the nineteenth century up to the recent emergence of the new African nations, trypanosomiasis has presented a complex and challenging problem over a wide field of endeavour. In the West, the emphasis has perhaps been chiefly on the human disease, in the East on the infection of animals, though both aspects are important throughout central Africa. The changes in the incidence of the disease have a complex relationship to social and political changes of the period. Proposed methods of control have been many, and sometimes conflicting. With so many disciplines concerned, an overall picture of the situation is opportune.

This book sets out to summarize the varied and voluminous work on trypanosomiasis in Africa from the discovery of the cause of the disease to the present day. A few of the 27 authors of the 43 chapters are still actively engaged on field work in Africa. Many have returned to their native lands. The authors are all English speaking but the work of the other ex-colonial powers is also covered. The names of a few African scientists appear in the references.

For the reader who specializes in none of the many aspects of this subject, the outstanding contribution is Duggan's "Historical Perspective". In 48 pages he outlines the problems of the whole subject, and progress towards their solution. This is the most scholarly contribution; the writing is clear and distinguished. The rest of the book discusses in detail the components of the subject, in four parts. The first two deal with causative organisms and the insect vectors, the third with human trypanosomiasis and the fourth with the trypanosomiasis of animals.

This approach has been successful, and has produced a reasonable division of the subject, thereby avoiding over-fragmentation into field and laboratory, research and administration or East and West; this could easily have occurred.

Some overlap is inevitable and some differences of opinion—the value of pentamidine prophylaxis, for example—is acceptable. Agreement on all aspects of this enormous subject is not to be expected.

These chapters detail the laboratory and field work carried out all over

Africa which has resulted in a reasonable degree of control. Much has been done but many questions remain unanswered. It is still hard to foresee the elimination of the trypanosome by the weapons at present available. Will the position deteriorate if the residual insecticides are proscribed?

The indexing is, perhaps, not wide enough for a reference work of this scope. It is possible to find the work of each author only from the chapter headings. There is no page index of the many references in the bibliography.

I searched for information on the incubation period of the trypanosome in the tsetse fly—a figure basic for a logical approach to control. I found it stated in the chapter on the epidemiology of Gambian sleeping sickness, but could find no reference to the work on which this figure was based in the earlier chapters which cover the basic aspects of the subject, the trypanosome and the fly.

The individual chapters present an admirable summary of the achievements of the period, however, and can stand as a suitable memorial to the authors and their predecessors. The trypanosomiasis are not defeated; the information and ideas in this book should provide a base for further attack.

M. J. COLBOURNE

Chromosomes Computed

[illegible]

(a)



(b)

How to use a computer to recognize a chromosome. Computer methods are being used increasingly to solve pattern recognition problems, both in the physical and biomedical sciences. This illustration, taken from *Advances in Computers*, Vol. 10 (Academic: New York and London, November 1970, £6.75), shows (a) digitization of a photograph of a chromosome provided by a 'FIDAC' flying-spot scanner, and (b) the picture reconstructed.

CORRESPONDENCE

Indian Brain Drain

SIR,—Out of nostalgia and loyalty I continue to read the Indian news sheet which the local Indian High Commissioner is kind enough to send me regularly. It appears that recently a new and successful export industry has arisen in frozen frogs' legs. Of course, nobody in India would ever consider eating frogs' legs, except perhaps in unavoidable circumstances, but they do go down well on gourmet tables in the West. But for this appreciation, the frogs would continue to rot in the swamps of India.

Now, Sir, may I commend the enterprise, and generosity of vision involved in this new industry, to the notice of those who worry about the brain drain from the developing countries: in particular, to Dr A. Parthasarathi (*Nature*, 230, 87; 1971). Just because a certain number of scientifically trained persons migrate and do well elsewhere does not prove that their countries could have used them professionally. By all accounts there is a large number of unemployed engineers and other technical personnel in India. Surely it could not be maintained that the laboratories in India were understaffed, or that scientists who are in that country have sufficient material support to be working at their most efficient. They could be overworked, certainly, but not in such activities that any increase in their numbers would improve the situation.

In any case, if in certain areas a need is felt, the governments could always arrange to have people specially trained in that area without setting up the kind of elaborate and generalized controls envisaged by Dr Parthasarathi.

The system of counsellors (advisers) and so on suggested in the latter part of his article has some curious echoes—of a certain mythical unshouderable burden which still bothers the conscience of many a well-meaning white man (to their eternal credit, of course) and of mercenaries fighting against national movements. It is just as well that nothing is likely to come out of it, in view of the present administrative and intellectual turmoil in the universities throughout the West.

The main problem is to make the modern intellectual enterprise a relevant and self-sustaining force in these countries and the profitable and modish export which does no harm to this should be allowed to continue. Surely the countries of their origin have benefited because, for example, Chandrasekhar,

Khorana and Salam were able to use the opportunities they found in other countries.

Yours faithfully,

KAILASH KUMAR

*The Australian National University,
Canberra*

Chemistry Teaching

SIR,—Your recent editorial criticism¹ of the report of the Committee of Enquiry which I chaired is seriously misleading because of the many errors of fact it contains, and I must ask for space to reply.

On the matter of broadening of courses you attribute to the committee opinions which it did not express and then criticize those opinions. Thus you point to our findings that some students would like to study a range of subjects outside chemistry and its essential ancillaries, and comment "The only puzzle is that the Eaborn Committee should dismiss the case for broader education without much concern for argument one way or the other". In fact, the clearly expressed view of the committee on this matter was that "It is highly desirable that chemistry students should have the option of spending some of their time outside chemistry and its essential ancillaries; this might be up to 20% of the three-year degree course in England and Wales. Our surveys reveal a substantial demand by students for courses in subjects such as economics and sociology, and we strongly favour such courses being available as options". You further note the wish of employers for inclusion in the course of commercial subjects (operational research, cost-accounting, marketing, industrial relations, computer programming, and so on), and state that "the committee takes the line that there is no time for luxuries like that". On the contrary, while we did, surely realistically, say that not *all* such subjects could be included in *every* chemistry course, we recommended that they should be available as options, saying in this context that "The differing inclinations of students can best be accommodated at present by setting aside time for a range of optional subjects other than chemistry and its essential ancillaries". Of computer programming, however, we stated unequivocally that "all chemistry students should receive instruction in this topic". Degrees in chemistry are already broader than those

in some other sciences, since, except in one university, they involve courses in physics and mathematics, so that, with the same exception, the student spends between half and two-thirds of his time on chemistry as such. If our proposals for setting aside 20% of the time for optional subjects outside chemistry and its essential ancillaries were adopted, along with our recommendation that computer programming be included in all the courses, the students who wished to take up the options would spend no more than 40%–50% of their three years on the study of chemistry.

You also criticize the committee for disregarding the complaints of many chemistry students that too much factual material has to be remembered and that the work load is too heavy. On the contrary, we called on university teachers to examine the factual content of courses with a view to reducing it, and added "While emphasis on the memorizing of facts has decreased markedly in recent years, it is likely that this movement still has some way to go", furthermore commenting that "Provided that the graduate emerges with an adequate background of information and understanding, the attitude the course has induced in him is much more important than his detailed knowledge". We also expressed the view that the heavy work-load in chemistry prevents many students from reaping the full intellectual benefits of the university environment, and called on teachers to "consider carefully whether the work load in chemistry cannot be significantly reduced without serious loss of coverage", pointing to ways in which cuts could be made. We furthermore recommended that the time spent in laboratory classes should be reduced to the minimum necessary to meet a limited aim which we defined.

I will not attempt to deal with all the remaining inaccuracies in your account, but make brief comments on only three of them, as follows: (i) There is no justification for your statement that the committee, after pointing out some advantages of the present courses, "deplores the way in which students appear to be increasingly unwilling to exploit these advantages". On the contrary, we drew attention to the students' viewpoints, and commented "University chemistry teachers must take account of the changing undergraduate attitudes, and ensure that their courses are in every way as attractive as possible to good students,

rather than seek to maintain established practices simply because they may seem to be correct from the narrowest viewpoint of immediate vocational training".

(ii) It is more difficult to counter your statement that the committee "bemoans the difficulties which beset chemistry graduates seeking jobs", because it is entirely fiction, and I can only challenge you to produce quotations from the report in support of your assertion. In the matter of job shortages you may have been misled by your Education Correspondent's statement (not taken from the report) that "chemistry graduates have consistently formed the largest proportion of unemployed scientists six months after graduation"². This is, in fact, the case (and not surprisingly so, since there are more graduates in chemistry than in any other science), but surely much more significant is the fact that in 1968 and 1969, the latest years for which official (UGC) figures are available, the percentage of chemists still unemployed six months after graduation was below the average for all science students, and in 1969, for example, the figure, 3.7%, was less than half of that in zoology (8.0%) or combined and general biological sciences (8.3%).

(iii) You write that we "respond to the general feeling among chemists (in industry) and employers that a capacity to write literate reports would be an asset" by "woodenly proclaiming that we cannot recommend that a formal course in report writing and the use of English should form part of all chemistry degree courses", and you add that we "pass the buck downwards to the secondary schools and upwards to induction training". We were not, contrary to your assertion, responding to an opinion that capacity to write literate reports would be an asset, which no one could possibly question, but to the view that *instruction* in report writing and use of English *should be included in all courses*, and as far as buck-passing is concerned you carefully omit

our comments that "the student's ability to write clear and correct English must be laid in the secondary schools, though university chemistry teachers can help the student build on these foundations", and "University teachers of chemistry should give thought to informal means of improving a student's ability to present a clear report", which we followed up by practical suggestions.

I hold the opinion, with you, that chemistry courses must be made more attractive to good students. But I cannot, as you do, condemn the present courses because 26% of the undergraduates disagreed with the statement that the present courses are "modern, lively, and up to date", since, given the spread of student inclinations and abilities, I think it likely that almost any course in any subject will be found deficient by this sort of proportion. Furthermore, while I, along with the rest of the committee, am strongly in favour of the broadening of chemistry courses along the lines indicated above, I am not aware of any justification for the view that making chemistry courses much more general will in itself greatly improve their attractions, and it is noteworthy that only 14% of the chemistry undergraduates surveyed considered that their courses involved too much specialization, while 21% considered that they involved too little. Something much more imaginative is needed, and at least one radically novel approach to undergraduate chemistry teaching has already been introduced in the light of the information presented by the committee^{3,4}.

A final point. Chemists, whom you condemn for their conservatism, should at least be given credit for commissioning and publishing a thorough sociological survey of the attitudes of chemists and chemistry students. There is not the slightest reason to believe that the deficiencies and dissatisfactions it reveals are confined to chemistry and, indeed, the Docksey Report gives a clear indication

that some of them are not. Non-chemists who seek to use the information in our report to attack chemists and chemistry courses might do well first to carry out similar surveys within their own disciplines to find out just how far their own houses are in order.

Yours faithfully,

C. EABORN

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University of Sussex,
Brighton BN1 9QJ,
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¹ *Nature*, **228**, 1242 (1970).

² *Nature*, **228**, 1244 (1970).

³ *Chemistry in Britain*, **6**, 330 (1970).

⁴ *Nature*, **228**, 649 (1970).

REGRETTABLY this letter, received early in January, has not been published before this for reasons connected with the postal strike and not through any fault of Professor Eaborn's.

The chief complaint of the leading article was that the committee had been complacent and that the report would have a conservative influence. Unfortunately the report is a quarry for quotations which support this view and which tend the other way. Thus the report says "We are not convinced by arguments in the Swann Report that graduates would be more useful to industry if they had studied a wider range of scientific subjects to a less advanced level than is customary now in the special degree. We do not accept that any possible gains would outweigh the loss of professional competence in the subject of specialization". The recommendations that there should be more options are introduced with the phrase "While there is no convincing case for broadening all chemistry degree courses . . .". It is good to have Professor Eaborn's assurance that the report is really a recipe for reform; the difficulty is the impression created in the report that reform could be accomplished without substantial change.

On the three specific points raised here: (i) This was a reference to the decline of recruitment to chemistry departments. The report says "This state of affairs is a matter for concern for the country in general . . .". (ii) The committee says that "it is true that the student image of industry is in some respects inaccurate and is not wholly favourable, but there are no grounds for believing that this is a consequence of anything in the degree course . . .". (iii) Can the blind lead the blind, even informally?—Editor, *Nature*.

Announcements

International Meetings

May 4-5, **Electrical Association for Women Conference**, London (Electrical Association for Women, 25 Foubert's Place, London W1V 2AL).

May 8, **Human Diseases in the Seventies**, London (The Conference Secretary, Joint Biology Committee, 2 Bramley Mansions, Berrylands Road, Surbiton, Surrey).

May 18, **Perspectives in Laboratory Animal Science**, London (The Honorary Secretary, Laboratory Animals Science Association, c/o Canterbury Biological

Laboratories Ltd, 45 Sandwich Road, Ash, Canterbury, Kent).

May 24-27, **New Trends in Educational Technology and Industrial Pedagogy**, Knokke (Secretariat, Lamoriniestraat 236, B-2000 Antwerp, Belgium).

June 2, **Presenting a Technical Lecture**, London (Institute of Electrical and Electronics Engineers, 2, Savoy Hill, London WC2).

June 4, **Two-phase Morphology of Block Copolymers**, Colchester (Professor M. Gordon, Department of Chemistry, University of Essex, Wivenhoe Park, Colchester, Essex).

June 7-9, **Evolution of Genetic Systems**, Upton, LI (Harold H. Smith, Brookhaven

National Laboratories, Upton, New York 11973, USA).

June 28-July 2, **World Energy Conference**, Bucharest (The Secretary, British National Committee, World Energy Conference, 5 Bury Street, St James's, London SW1).

July 5-8, **Shock Tubes**, London (Symposium Secretary, Department of Aeronautics, Imperial College, Prince Consort Road, London SW7).

September 21-October 2, **Methods of Prospecting for Uranium Minerals** (NATO Advanced Study Institute), Imperial College London (6 day course of lectures) and Cornwall (2 days of field work) (Dr Michael Davis, UKAEA, 11 Charles II Street, London SW1).

British Diary

Tuesday, April 27

Solid State Microwave Devices (6.30 p.m.) Mr E. B. Callick, Institution of Electrical Engineers, London Graduate and Student Section, at Willesden College of Technology, London NW10.

Lead Mining in the Yorkshire Pennines (7.45 p.m.) Mr B. Jennings, Leeds Metallurgical Society, at the Houldsworth School of Applied Science, The University, Leeds.

Soil Metabolism (10 a.m.) Society of Chemical Industry, Agriculture Group, at 14 Belgrave Square, London SW1.

Communications through Space, Flame and Fibre (6 p.m.) Professor P. J. B. Clarricoats, Institution of Electronic and Radio Engineers, at the London School of Hygiene and Tropical Medicine, Keppel Street, Gower Street, London WC1.

Wednesday, April 28

Applications of CAMAC (5.30 p.m.) Mr H. Bisby, Institution of Electrical Engineers, at Savoy Place, London WC2.

The Economics of Farming (2.30 p.m.) Royal Society of Arts, at John Adam Street, London WC2.

Radio Meteorology (6.30 p.m.) Dr J. A. Saxton, Institution of Electrical Engineers, at King Edward VI Grammar School, Broomfield Road, Chelmsford, Essex.

Plasma—The Fourth State of Matter (7.15 p.m.) Dr J. E. Allen, Institution of Electrical Engineers, at the High Wycombe College of Technology and Art, High Wycombe, Bucks.

Thursday, April 29

Design and Application of Minimal Computers (5.30 p.m. colloquium) Joint IEE/IERE Computer Group, at the Institution of Electrical Engineers, Savoy Place, London WC2.

Some Problems with Marine Propellers in Service (7 p.m.) Mr A. W. O. Webb, Liverpool Metallurgical Society, in the Department of Metallurgy and Materials Science, at the University, Liverpool.

Direct Digital Control—The Case for the Special Purpose Computer (7.30 p.m.) Mr P. Atkinson and A. J. Allen, Institution of Electronic and Radio Engineers, at the J. J. Thomson Laboratory, University of Reading, Whiteknights Park, Reading, Berks.

An Alternative to FDM/TDM Code Division Multiplex (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Friday, April 30

Statistical Model Building, Prediction and Control (10 a.m. colloquium) Institution of Electrical Engineers, jointly with the Operational Research Society and the Institute of Measurement and Control, at Savoy Place, London WC2.

The French Chemical Industry (6.30 p.m.) M. J. Moundlic, Society of Chemical Industry, London Section, at 14 Belgrave Square, London SW1.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

National Institute of Industrial Psychology. *Annual Report and Statement of Accounts for the year ended 30th September, 1970*. Pp. 30. (London: National Institute of Industrial Psychology, 1971.) [193]

Other Countries

Public Health Implications of Radioactive Waste Releases. By C. P. Straub. Pp. 61. (Geneva: World Health Organization; London: HMSO, 1970.) 8 Sw. francs; 80p; \$2.75. [181]

The Walter and Eliza Hall Institute of Medical Research, 1969/1970. Pp. 103. (Melbourne: The Walter and Eliza Hall Institute of Medical Research, 1970.) [191]

US Department of the Interior: Geological Survey. *Water-Supply Paper 1963: Quality of Surface Waters of the United States, 1965*. Parts 5 and 6: Hudson Bay and Upper Mississippi River Basins, and Mis-

souri River Basin. Pp. xi+548. (Washington, DC: Government Printing Office, 1970.) \$2.50. [191]

Australia: Commonwealth Scientific and Industrial Research Organization. CSIRO. *Twenty-second Annual Report 1969/1970*. Pp. 104. Division of Soils—Report on Progress, 1969. Pp. 65. Annual Report of the Division of Chemical Physics, 1969/1970. Pp. 52. (Melbourne: CSIRO, 1970.) [191]

The Year Book of the Indian National Science Academy (Formerly National Institute of Sciences of India), 1970. Pp. xiii+223. (New Delhi: Indian National Science Academy, 1970.) 15 Rs. [191]

Behavioral Sciences and Mental Health: An Anthology of Program Reports. Edited by Eli A. Rubinstein and George V. Coelho. (US Department of Health, Education and Welfare—Public Health Service. Health Services and Mental Health Administration.) Pp. viii+419. (Washington, DC: Government Printing Office, 1971.) \$2. [33]

US Department of Commerce—National Bureau of Standards. NBS Special Publication No. 338: Status of Thermal Analysis—Proceedings of a Symposium on the Current Status of Thermal Analysis, held at Gaithersburg, Maryland, April 21–22, 1970. Edited by Oscar Menis. Pp. iv+185. (Washington, DC: Government Printing Office, 1970.) \$1. [33]

South Africa's Contribution to Science. By Dr S. Meiring Naudé. Pp. 26. (Cape Town and Johannesburg: Tafelberg-Uitgevers, 1970.) [83]

Annals of the Transvaal Museum. Vol. 26, No. 12: New Herpetological Records from South West Africa. By W. D. Haacke. Pp. 277–284. Vol. 26, No. 13: Evaluation of Springbok-Like Fossils—Measurement and Statistical Treatment of the Teeth of the Springbok, *Antidorcas marsupialis marsupialis* Zimmerman (Artiodactyla: Bovidae). By E. S. Vrba. Pp. 285–300. (Pretoria: Transvaal Museum, 1970.) [83]

Canada: Department of Energy, Mines and Resources. Geological Survey of Canada. Paper 70–39: Surficial Geology of the Rustico Map-Area, Prince Edward Island. By G. H. Crowl and L. Frankel. Pp. v+15. \$2. Paper 70–51: Bedrock Topography and Geology of Southern Manitoba. By R. W. Klasmann, J. E. Wyder and B. B. Bannatyne. \$0.75. Paper 70–52: Radioactivity in Western Canadian Coals. By A. R. Cameron and T. F. Birmingham. Pp. vi+35. \$2. (Ottawa: Queen's Printer, 1970.) [93]

Republic of South Africa: Department of Industries. Division of Sea Fisheries Investigation Report No. 91: Age and Growth of the South African Pilchard, *Sardinops ocellata*. By D. Baird. Pp. 16. (Sea Point, Cape Town: Division of Sea Fisheries, 1970.) [93]

Science Council of Canada. Report No. 11: A Canadian STOL Air Transport System as a National Major Program. Pp. iii+38. (Ottawa: Science Council of Canada, 1970.) [93]

Food and Agriculture of the United Nations. Requirements of Ascorbic Acid, Vitamin D, Vitamin B₁₂, Folate, and Iron. (Report of a Joint FAO/WHO Expert Group. WHO Technical Report Series, No. 452.) Pp. 75. (Rome: FAO; London: HMSO, 1970.) 8s.; \$1.25. [103]

Bulletin of the American Museum of Natural History, Vol. 142, Article 6: A North American Oligocene Pangolin and other Additions to the Pholidota. By Robert J. Emry. Pp. 455–510. (New York: American Museum of Natural History, 1970.) \$2.50. [103]

Tissue Antigens, Vol. 1, No. 1, 1971. Edited by F. Küssmeyer-Nielsen. Pp. 1–52. Subscription price: D.kr. 150.00 plus postage D.kr. 10.00 (\$21.50) per volume payable in advance. Price per issue D.kr. 30.00 (\$4.00). (Copenhagen: Munksgaard, 1971.) [113]

Bulletin of the American Museum of Natural History, Vol. 143, Article 4: Oyster-Like Permian Bivalvia. By Norman D. Newell and Donald W. Boyd. Pp. 217–282. (New York: American Museum of Natural History, 1970.) \$2.75. [113]

Parliament of Fiji. Parliamentary Paper No. 30: Report of the Department of Agriculture for the year 1969. Pp. 37. (Suva: Department of Agriculture, 1970.) 50c. [123]

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